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General Astronomy.

ON THE PHYSICAL CONSTITUTION OF THE PLANET JUPITER.*

G. W. HOUGH.

The planet Jupiter was one of the first objects to which the telescope of Galileo was directed, and the satellites of the planet were among the earliest discoveries made with that instrument. In 1630, the telescope had been constructed of sufficient power to show the great equatorial belt, and previous to the beginning of the 18th century, the principal phenomena seen on the surface of Jupiter had been observed and the time of rotation and the position of the axis of the planet ascertained. Notwithstanding, however, the great mass of facts which have been collected from observations extending over a period of two hundred and fifty years, yet, up to the present time no theory of the physical condition of the surface has been advanced which has met with universal acceptance.

It is not our purpose to describe in minute detail Jovian phenomena, but simply to call attention to a few points which have a direct bearing on the topic under consideration.

In order that the subject may be more clearly understood, it will be well to state briefly the salient features presented to the eye of the observer. The disk of Jupiter appears as an ellipse, having axes in the ratio of 14 to 15; the longer axis lying in the direction of the planet's equator. The equatorial diameter is about 89,000 miles, and 1″ of arc, seen from the Earth at mean distance, represents 2,300 miles. The mean density of the planet is 1.37 times that of water, and hence the surface density is probably less than that of any known liquid. As the equator of the planet is inclined only three degrees to the orbit, the effect of the Sun will be nearly constant through the Jovian year, and phenomena due to meteorological conditions should have great permanency. During the revolution of the planet in its orbit, the

* Paper read before the Congress of Astronomy and Astro-Physics held in Chicago, August, 1893.

equator as seen from the Earth, is displaced about four degrees, or 1″ of arc. The objects, therefore, seen near the center of the disk, may apparently be shifted in latitude plus or minus 1″ of arc, while objects in higher latitude will be displaced a less amount from the same cause. As the observer is looking on the surface of a sphere, objects will only be seen in normal proportions when on the middle of the disk. At the edge of the disk they will be infinitely small. The rotation of the planet, therefore, will cause all objects to change their size and shape as they are brought under the eye of the observer. Since the apparent rotation of the surface is most rapid at the equator and zero at the poles, objects passing across the disk will appear to move with different linear velocities, so that spots or markings, lying in different latitudes, may apparently change their relative positions under the eye of the astronomer; a fact which is sometimes lost sight of by modern observers.

There are two principal periods of rotation, determined by the observation of spots and markings, which in round numbers are $9^h\ 56^m$ and $9^h\ 50^m$.

If the longer period is assumed as the approximate time of the rotation of the planet, the proper motion of the objects conforming to the shorter period is about 250 miles per hour at the equator, or a complete revolution in 45 days. Neither of the periods given are absolutely fixed, but the observed motion of markings approximately conforms to the one or the other. The rotation period appears to be independent of the latitude of the object observed.

The most conspicuous marking on the disk is the great equatorial belt, which has been visible since the earliest observations with the telescope. The rotation period from observations of the belt, is $9^h\ 56^m$. The belt changes in size and position, expanding or contracting in width. On either side of the equator there are other belts which are not so distinct, but all are arranged approximately parallel to the equator of the planet. Dark spots or markings are frequently seen on these belts. These spots usually have a drift in longitude relative to each other. The spots are sometimes seen without material change in size or shape during two or more oppositions of the planet.

In latitude 8″ to 12″ south, oval white spots have been observed by myself at every opposition since 1879. Similar objects have been delineated by earlier observers. These spots are usually from 1″ to 1.5″ of arc in diameter; they have motion among themselves in longitude and possibly in latitude.

Now, although we only see objects in two dimensions, it is reasonable to suppose from their shape that they have a depth commensurate with their surface dimensions, in which case, these oval spots may extend downwards towards the center of the planet at least 3,000 miles. Since these oval spots are free to move with reference to each other, it indicates that the medium in which they are located, has a depth at least equal to the diameter of the object.

The most conspicuous isolated mass of dark matter, is the great red spot, south of the equator, which has excited so much interest since 1878. It appears probable that this object was observed by Cassini in 1665, and also by modern observers previous to 1878. Between 1665 and 1713 the ancient spot reappeared and vanished nine times and at no period was it visible for more than three years. The spot is elliptical in outline, having a length of about 30,000 miles and a breadth of 8,000 miles.

The determination of the period of rotation of the planet, in 1879 and subsequent years, from its observation, indicates that the spot is not stationary but has a slow drift in longitude. There has also been a slight shifting in latitude. Now, if the depth of the spot is assumed equal to its breadth, it would indicate that the medium in which it floats extends downwards at least 8,000 miles. The great change in the color and visibility of the spot during the past fourteen years, would be explained by its greater or less submergence beneath the surface.

Granting, therefore, the reasonable assumption that the oval, nearly round detached objects, are not simply superficial in their dimensions, we must conclude that the medium in which they are located has a depth measured by thousands of miles.

The satellites of Jupiter also offer phenomena which have a direct bearing on the subject. The satellites at times cross all parts of the disk in transit. Usually the satellite disappears at some distance from the limb after ingress and reappears at a similar distance before egress. From this fact it is concluded that the center of the disk of Jupiter has the same reflecting power as the satellite, and hence has no inherent light of its own.

Last year it occurred to me to ascertain definitely at what distance from the limb of the planet the satellite would disappear when projected on the disk in transit. From numerous micrometrical measures, I ascertained that a satellite could be followed with the 18½-inch refractor to a distance of 10″ of arc from the limb. When the transit, however, occurred within 10″ of the pole, (3 and 4 sat.) the satellite could be seen during the entire

transit across the disk. Now, if the diminution of light at the limbs of the planet is due to atmospheric absorption, it would seem to indicate an enormous atmosphere, one of about 20,000 miles in depth. But from the well defined outlines of the limbs of the planet, most astronomers have concluded that the true atmosphere can have no great depth as compared with the diameter of the planet.

From what has already been said regarding the probable magnitude of objects and their freedom *inter se*, it seems to me that all the phenomena observed can be best accounted for by assuming that the planet is yet in a gaseous condition.

Recapitulation:

The arguments leading to the conclusion that the planet is gaseous, are two:

1st. The probable volume of the spots both white and dark, seen on the disk and their freedom of motion *inter se*.

2d. The gradual fading of the light of the satellite when projected on the disk in transit.

LIGHT-WAVES AND THEIR APPLICATION TO METROLOGY.

A. A. MICHELSON.

Every accurate measurement of a physical quantity depends ultimately upon a measurement of length or of angle; and it will readily be admitted that no effort should be spared to make it possible to attain the utmost limit of precision in these fundamental quantities. At present, lengths are measured by the microscope, and angles by the telescope; and the extraordinary degree of accuracy already attained by the use of these instruments depends entirely on the properties of their optical parts in their relation to light-waves; so that, in fact, light-waves are now the most convenient and universally employed means we possess for making accurate measurements. It can readily be shown that this high degree of accuracy is especially due to the extreme minuteness of these waves.

Thus it is well known that the image of a luminous point consists of a series of concentric colored rings surrounding a bright central disc which is smaller the smaller the ratio of the wavelength of the light to the diameter of the objective employed. In fact, it can be shown that the radius of the bright central disc

* From *Nature*, Nov. 16, 1893.

contains as many wave-lengths as the distance of the image from the objective contains the diameter of the objective. Thus in a telescope twenty diameters long, the diameterof the bright disc is forty wave-lengths or 0.02 mm. If the image be magnified by increasing its distance from the objective, or otherwise, these diffraction rings are magnified in the same proportion; so that nothing is gained thereby in *distinctness*, beyond the point where the rings are just large enough to be visible. But, were it not for the inevitable loss of light, it would be advantageous for *measurements of position* to increase the magnification much further.

This can be accomplished by an extremely useful instrument which has been misnamed the "interferential refractometer." It will be interesting to note that notwithstanding the apparent difference in form, this apparatus, when used as a measuring instrument, differs in no essential particular from the microscope or the telescope, or (what is perhaps a trifle unexpected) the spectroscope; and it is possible to change any one of these instruments into the other by unimportant modifications.

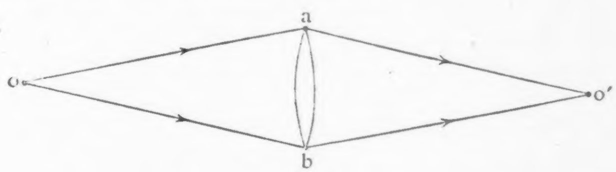


FIG. 1.

Thus, let *o*, Fig. 1, be a source of light, *a b* a lens which forms an image of *o* at *o′*. The operation of the lens, when used to distinguish minute objects, depends upon the accuracy with which all its parts contribute to make the elementary waves reach the focus in the same phase of vibration; but to determine the position of *o* with respect to *a b*, this is not at all necessary; and in fact, if we disregard the possible inconvenience due to the dissimilarity between the phenomenon observed and the object whose position is to be measured, it would be as well to entirely annul the central portions of the lens, leaving only an external annular ring, or better still, only two small portions at opposite ends of a diameter.

This involves no sacrifice of accuracy, but on the contrary a very considerable gain; for it is now possible to increase the size of the interference fringes up to any desired limit without diminishing the intensity of the light, the result being the same as could be obtained with a perfect microscope of unlimited magnifying power with a source of unlimited intensity.

For this purpose the two small portions to which the lens is reduced are replaced by plane mirrors or prisms, whose office is simply to bring the two interfering pencils into coincidence. Further, the pencils, instead of starting from a point or a line, may be separated by a plane transparent surface; and a second similar surface may be used to reunite the pencils after reflection. Thus the telescope or microscope will have been converted into a refractometer. The exact nature of the analogy will be apparent by a comparison of Figs. 1 and 2.

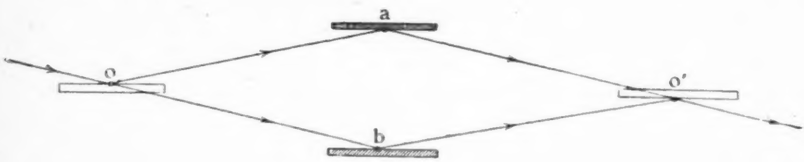


FIG. 2.

It may be assumed that under the most favorable circumstances the utmost attainable limit of accuracy of a setting of the cross-hair of a microscope on a fine ruled line is about $\frac{1}{20}$ of a micron. Now it is usually admitted that the middle point of an interference fringe, if it be sufficiently broad and clear, can be determined within about $\frac{1}{30}$ of the width of a fringe. In the refractometer this would mean only $\frac{1}{60}$ of a light-wave, or about 0.01μ from which it would follow that the refractometer is about five times as accurate as the microscope. But a number of trials with the form of refractometer shown in Fig. 8 gave as the mean error of a series of ten observations:

Fr.	Fr.	Fr.
Morley 0.0056 ...	Nicholson 0.0059 ...	X 0.0110

The third observer had no previous practice in this kind of measurement.

It is evident from these results that $\frac{1}{30}$ of a fringe is too large an estimate of the average error of a setting, and that it is, in fact, less than 0.01 of a fringe, corresponding to an error in distance of about 0.003μ.

For angular measurement the microscope is replaced by the telescope.

Fig. 3 represents a disposition sometimes adopted for observing minute angular displacements of the mirror *d c*; the light starts from *o*, is reflected by the plane parallel glass plate *p* to the objective *a b* of a telescope, whence the now parallel rays proceed to the mirror *c d*. Thence they retrace their path to the plate *p*,

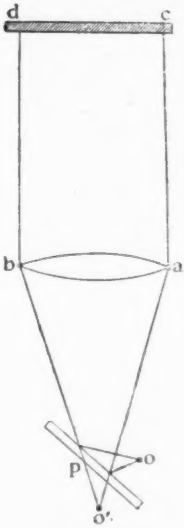


Fig. 3.

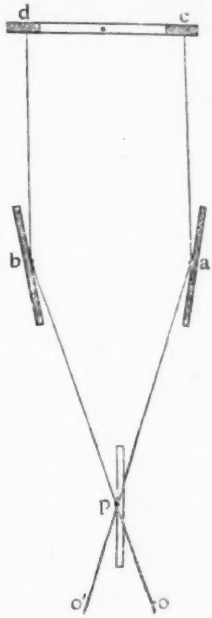


Fig. 4.

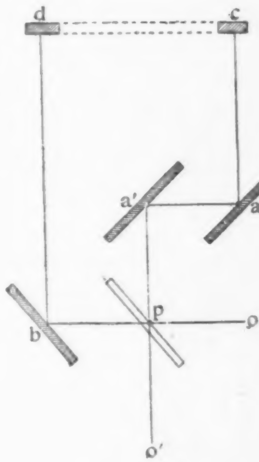


Fig. 5

through which they are transmitted, forming an image of the source at *o'*, which is viewed through the eyepiece.

Fig. 4 is the exact analogue in the form of a refractometer; and Fig. 5, though slightly different in aspect, is still essentially the same instrument. The path of the rays is *o p a c a p o'* for one of the pencils, and *o p b d b p o'* for the other.

From considerations quite analogous to those employed in the former case, it can be shown that the limit of accuracy attainable in the estimations of angles involves an error of about one-fifth of the angle subtended by a light wave at a distance equal to the diameter of the objective. This is halved by the fact that the angular motion of the beam is twice that of the mirror; so that with a telescope of 10 cm. aperture the limit of accuracy may be estimated at $\frac{1}{2000000}$, or say 0.1″. But taking 0.01 fr. as the smallest perceptible displacement of the mirrors *cd*, the corresponding angle of rotation of the line *cd* (10 cm. long) would be only $\frac{1}{20000000}$, or say 0.01″.*

* In the use of the revolving mirror as in galvanometers, gravity and torsion balances, etc., the accuracy can be increased by enlarging the surface of the mir-

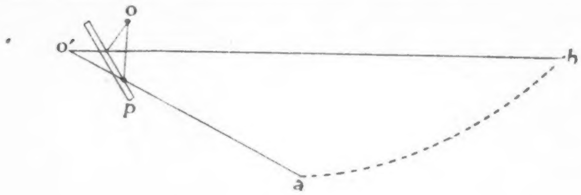


FIG. 6.

It is not at first evident that there is any relation between the refractometer and the spectroscope. A comparison of Fig. 6 and Fig. 7 shows, however, that there is a strict analogy. Fig. 6 represents a disposition sometimes adopted to observe the spectrum by means of a concave grating, and Fig. 7, with unimportant modifications, is the arrangement actually employed in the analysis of radiations by means of their "visibility curves," as will be explained below.

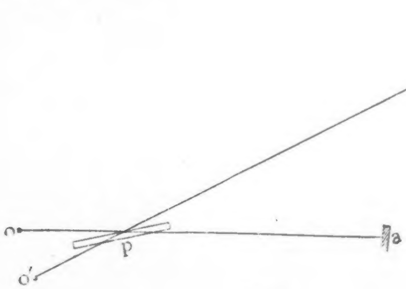


FIG. 7.

Exactly as in the case of mirrors and lenses, we may here, too, sacrifice "resolution" and "definition" by using only the extreme portions of the surface, with an actual gain in "accuracy." To compare numbers, it appears that the average error in the comparison of wave-lengths by a grating with 250,000 lines is about one part in half-a-million. With this number of waves in the difference of path of two interfering pencils, the corresponding error in the refractometer observations are of the order of one twenty-millionth.

ror; but the moment of inertia is thereby increased, and in greater proportion. But in the refractometer the mirrors *cd* may be made insignificantly small, and yet, with the same distance between the outer edges, the accuracy may be increased at least tenfold. It is important to note that any linear motion of the line joining the mirrors, or even a rotation about this line, has no effect on the fringes. It seems probable that this form of instrument may be of service in such problems as the measurement of the Moon's attraction, constant of gravitation, variations of the vertical, etc.

The name "interferential refractometer" seems rather inappropriate to an instrument which has so many important applications beside the measurement of indices of refraction; but as it has been sanctioned by long usage it will be retained.

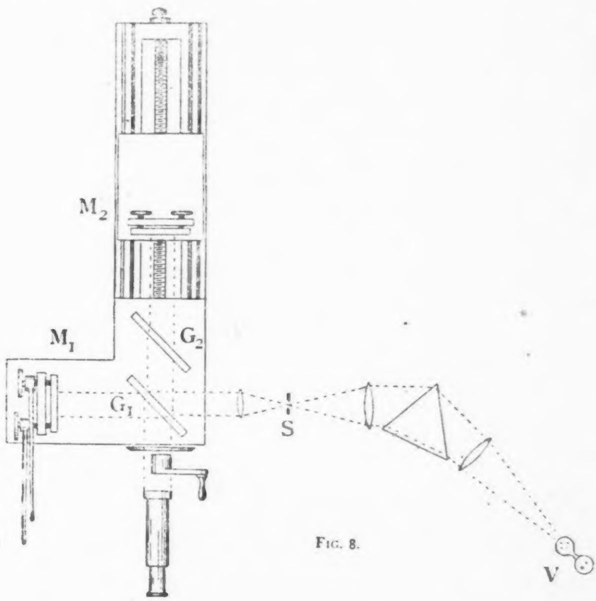


Fig. 8.

Among the many forms of the apparatus which have been rendered classic by the works of Arago, Fresnel, Fizeau, Jamin and Mascart, and which are so admirably adapted to the work for which they were designed, there are none which are not open to serious objections when applied to the solution of such problems as the measurement of lengths and angles, for the analysis of the constitution of the light of spectral lines, and especially for the determination of wave-lengths in absolute measure. For these, the form of instrument shown in Fig. 8 has many important advantages, among which the following may be mentioned:—It is simple in construction, and is easily adjusted; it may be used with a broad luminous surface as source of light; the pencils may be separated as far as desired; its range of difference of path between the interfering pencils is unlimited; and when properly adjusted the position of the interference fringes is perfectly definite, so that there is no uncertainty on account of parallax, and no

difficulty in counting the number of fringes passing a given point. Finally, it may be added, that this is probably the only form of instrument which permits the use of white light (and consequently of the identification of the fringes) in the determination of the position or inclination of a surface without risk of disturbance due to contact or close approximation.

As shown in Fig. 8, the refractometer consists essentially of a plane parallel plate of optical glass G_1 and two plane mirrors M_1 M_2. The beam of light to be examined falls on the plate G_1 at an angle, usually 45°, part being reflected and part transmitted.* The reflected portion is returned by the mirror M_2, and passes back through the inclined plate. The transmitted portion is returned by the mirror M_1, and is reflected by the inclined plate, and from this point it coincides with the other beam, so that the two are in condition to produce interference fringes.†

A little consideration will show that this arrangement is in all respects equivalent to an air-film or plate between two plane surfaces. If the virtual distance between these surfaces is small, white light may be employed, and interference fringes may be observed similar in all respects to those between two plates of glass pressed nearly into contact.‡

If, however, the distance exceeds a few wave-lengths, monochromatic light must be employed. In this case the fringes are in general invisible, unless they be viewed through a small aperture. If, however, the two surfaces are very accurately parallel, the fringes are always distinct, and it follows from the symmetry of the conditions that they are concentric rings. Their diameters increase as the square root of the order of the ring.

These rings are not formed at the surface of the mirrors (as is the case when the distance between them is small), but are per-

* The front surface of the plate G_1 is lightly coated with silver. The light which leaves the refractometer is a maximum where the thickness of the silver film is such that the intensities of the transmitted and reflected portions are equal. The silvering has another important advantage in diminishing the relative intensity of the light reflected from the other surface; and for this reason the thickness of the film may be advantageously increased, which permits also a more uniform surface. The ultimate ratio of intensities of the two pencils is not affected, for what is lost by transmission on entering the plate is made up by reflection on leaving it.

† One of the beams has to pass twice through the thickness of the glass plate G_1, and in order to equalize the two paths, a similar plate G_2 is introduced in the path of the other beam.

‡ If the plate G_1 be not silvered, the colors follow the same order as those of Newton's rings, but if the silvering be sufficiently heavy, the colors are complementary; this, if the plates G_1 and G_2 are exactly equal and parallel. Otherwise, the excess of path in glass of one of the pencils disturbs the order of colors by the effect of achromatism due to the dispersion of the glass, as was first pointed out by Cornu.

fectly distinct when the eye or the observing telescope is focussed for parallel rays.

In the preceding comparison between the refractometer and the telescope, microscope, or spectroscope, the "accuracy" has been increased at the expense of "definition." When, however, the object viewed is beyond the "limit of resolution" of the instrument, its form and distribution of light can no longer be inferred from that of the image. Thus, if the object be a disc, a triangle, or a double star, the appearance in the telescope is the same. Similarly in the spectroscope, a source of great complexity cannot be distinguished from one which produces a single spectral line. So that for such objects, even in the ordinary sense of the word "definition," the more familiar optical instruments cannot claim any advantage over the refractometer; but if by "definition" is meant not the actual resemblance of the image to the object, but the accuracy with which the form or the distribution of light in a minute source may be inferred, then it can be shown that all the advantage rests with the refractometer.

As an illustration of such an application of interference methods, let us consider the celebrated experiment of Fizeau, in which Newton's rings are observed with a sodium flame as source. The light, consisting of two separate systems of radiations differing by about one-thousandth in wave-length, each system produces its own series of interference fringes. When the surfaces are nearly in contact, the difference of path is very nearly the same for both systems, and the fringes coincide, and the clearness is a maximum. When, however, the difference af path reaches about 500 waves for one of the systems, it is a half wave more for the other; and the maxima of intensity of the one coincide with the minima of the other; hence at this point the fringes are faintest. But when the difference of path of the first system is about 1000 waves, it is a whole wave more for the second, and the fringes coinciding, there is again a maximum of distinctness. M. Fizeau has counted 52 such periods, corresponding roughly to a difference of path of 50,000 waves.

Suppose, now, that this double line were so close that it could not be resolved by the spectroscope; then from the evidence furnished by the variations in distinctness of the interference fringes as the difference of path increases, the duplicity of the line could be readily detected. But besides this, it can be shown that the relative intensities of the components, their distance apart, and even the distribution of intensities within the component lines can be inferred.

Thus it has been shown (*Philosophical Magazine* for September, 1892) that among some twenty radiations which were examined (though all give simple lines in the spectrum) the great majority of them are shown to be highly complex. Thus, the red hydrogen line is a double whose components have the intensity ratio 7 : 10, and whose distance is about a fiftieth of the interval between the sodium lines. Each component of the yellow sodium lines is itself a double whose components are in the ratio 7:10, and whose distance is about one-hundredth of that between the principal components. Thallium gives a double line whose components are in the ratio 1 : 2, at a distance of about a fiftieth of that of the sodium lines, while each component has a small companion whose intensity is about a fifth of that of the principal lines, at a distance of about one three-hundredth of that of the sodium lines.

The green mercury line is made up of a group of five or six lines, the strongest of which is itself double (or perhaps triple) the distance of the components, being less than a five-hundredth part of that between the sodium lines.

These distances, small as they are, can be measured within about a twentieth part, so that by this means it is possible to detect a change of wave-length corresponding to the ten-thousandth part of that between the two sodium lines.

The red line of cadmium is the simplest of all the radiations thus far examined, consisting of a single narrow line whose intensity falls off symmetrically according to an exponential law, its width (at the points where its intensity is reduced to half its maximum value) being only 0.002 (D_1-D_2). The green and the blue cadmium lines are also comparatively simple, and all three of these lines give interference fringes clearly visible at a difference of path of 100 mm., and under appropriate conditions they all satisfy the requisites for a definite and inalterable standard of length.

The most important of these conditions is that the radiating vapor be so rare that the molecules may vibrate freely; in other words, that the time occupied in the collisions between the molecules be so short relatively to that of the free path, that its influence in disturbing the free vibration may be neglected. Experience shows that in general this limit corresponds to a pressure of one or two thousandths of an atmosphere.

It may be noted that at atmospheric pressure—even when the radiating substance is introduced in quantity barely sufficient to color a Bunsen flame—the greatest difference of path attainable

is only one or two centimetres, whereas with mercury vapor in a vacuum tube intereference fringes have been observed with a difference of path of 47 centimetres, or about 850,000 waves.

In order to make any practical use of these minute quantities for standards of length, it is necessary to employ an intermediate standard, such as that shown in Fig. 9 consisting of a bronze bar carrying two plane-parallel glasses, silvered in front, the distance between which can be compared on the one hand with the fundamental standard in actual use—the metre or the yard—and on the other with the length of a light-wave.

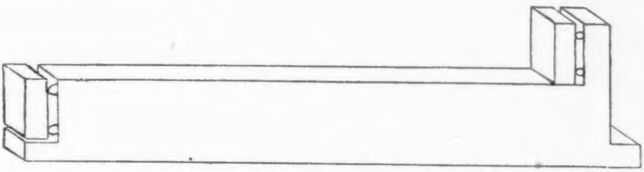


FIG. 9.

The former process is accomplished by moving the standard (whose length it is convenient to take at 10 centimetres) ten times through its own length, the coincidence and the parallelism of the surfaces being controlled at every step by the interference fringes in white light formed between these surfaces and that of the *reference plane* (the virtual image of the mirror MM in G_1, Fig. 8.) The position of a fiducial mark on this standard is compared by means of two micrometer microscopes with the lines defining the standard metre at the first and last steps.

In the second process the only difficulty encountered is due to the very great disproportion between the length of a wave and that of the 10 centimetre standard, and the consequent difficulty in keeping the correct count of the very large number of waves which pass as the reference plane is moved from one surface to the other.

This problem has been solved in the following manner. Nine standards were constructed similar in all respects to that of ten centimeters, save that each succeeding one was half as long as the preceding. The last of the series is thus approximately 0.39 mm. long, corresponding to a difference of path of 0.78 mm. The number of waves in this distance in red cadmium light is 1212 plus a fraction, which is corrected by direct observation of the difference of phase of the circular fringes on the upper and lower (front and rear) surfaces of the standard. This verification is also made with the green and blue radiations.

It is important to note that the measurement of these fractions alone is sufficient to fix the whole number, even if there be an uncertainty of several waves. Thus the relative wave-length of the three radiations being known, the number of green and of blue waves corresponding to the observed number of red waves can be readily calculated, as is shown in the following table:—

Wave-length	Number of Waves	
μ	Observed	Calculated
0.64389	1212.34	1212.34
0.50863	1534.76	1534.76
0.48000	1626.16	1626.13

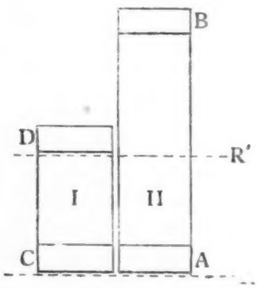


Fig. 10.

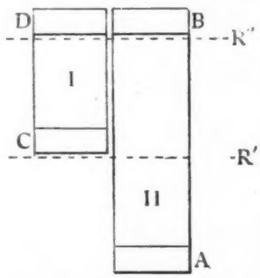


Fig. 11.

If the whole number assumed as the basis of this calculation were in error by one or more waves there would be no correspondence between the observed and calculated fractions. The length of this standard and the succeeding one are now compared as follows:—The two standards being placed side by side in the refractometer II on a fixed support, and I on a movable carriage, the reference plane (R, Fig. 10) is moved until it coincides with A the lower (or front) surface of II, and the interference fringes in white light are adjusted to the proper distance and inclination by adjusting the inclination of the reference plane. Next C, the lower surface of I is brought to coincidence with the reference plane and similarly adjusted, and then all the adjusting pieces are released from the carriages, so that these rest undisturbed on the ways. This completes the *first stage* of the comparison.

Second Stage.—The reference plane R is now moved back till it coincides with D, the upper surface of I and the adjustment of the interference fringes carried out as before.

Third Stage.—The standard, I, is moved back till its lower sur-

face (C Fig. 11) once more coincides with the reference plane R′, and its inclination is again adjusted by the interferences fringes.

Fourth Stage.—The reference plane is finally moved back till it coincides with D, the upper surface of I, and its inclination is again adjusted. If now the standard II is just twice as long as I, the fringes will appear simultaneously on *both* upper surfaces D and E.

The adjustment of the length of the standard is usually made to within a few waves, and the outstanding difference is measured by a compensating device.

This is furnished by the rotation of the compensating plate G_2, Fig. 8. The plate is held in a metal frame which is supported at one end by a short thick rod firmly fixed to the bed. At the other end a delicate spiral spring is attached; the tension of the spring *twists* the rod through a minute angle and thus alters the thickness of glass traversed by one of the interfering pencils. The other end of the spring is attached to a flexible cord passing over a pulley which is connected with a graduated circle. The angular motion is thus reduced about 100,000 times, and yet the proportionality is preserved.

Suppose the outstanding difference is ε a fraction of a wave-length known to within one or two tenths, then

$$\mathrm{II} = 2\,\mathrm{I} + \varepsilon$$

and consequently the number of red waves should be

$$2 \times 1212.34 + \varepsilon.$$

This fraction is corrected by direct observation, as in the case of standard I, and the same control is furnished by the concordance of the results for the three colors; so that an error in the whole number of waves is well-nigh impossible.

The process of comparison and correction is repeated in the same way with the other standards, until we finally arrive at the whole number of waves and approximate fraction in the 10 centimeter standard. Up to this point the question of temperature and pressure is of minor importance for the comparisons and corrections are made while both standards are under the same conditions; and being all made of the same material, it is sufficient to know that the temperature is the same for both. In the measurement of the fractions on the 10 centimetre standard, however, it is necessary to know the temperature and pressure with all possible accuracy, and it is also important that the comparison of this standard with the metre should be made, as nearly as

may be, under the same conditions as that of the determination of the standard in light-waves.

The author having been honored by an invitation from the International Bureau of Weights and Measures to undertake a series of experiments upon the lines here briefly indicated, the necessary apparatus was constructed in America, and shortly afterwards installed in the Bureau International des Poids et Measures at Sèvres.

Two complete and entirely independent determinations were made. These have not yet been completely reduced, but an approximate calculation gives for the number of waves of red light in one metre of air at 15° C and 76 mm.

First series.	1553163.6
Second series.	1553164.6

The difference from the mean is half a wave, or about one fourth of a micron.*

From these results it follows we have at hand a means of comparing the fundamental standard of length with a natural unit—the length of a light-wave—with about the same order of accuracy as is at present possible in the comparison of two metre bars.

This unit depends only on the properties of the vibrating atoms of the radiating substance, and of the luminiferous ether, and is probably one of the least changeable quantities in the material universe.

If therefore, the metre and all its copies were lost or destroyed, they could be replaced by new ones, which would not differ from the originals more than do these among themselves. While such a simultaneous destruction is practically impossible, it is by no means sure that, notwithstanding all the elaborate precautions which have been taken to insure permanency, there may not be slow molecular changes going on in all the standards; changes which it would be impossible to detect except by some such method as that which is here presented.

* The error in the determination of the *relative* wave-lengths of the three radiations is very much smaller, probably less than one twenty-millionth.

WEST INDIAN HURRICANES AND SOLAR MAGNETIC INFLUENCE.

H. A. HAZEN.

In the January number of this journal there was a paper by Professor F. H. Bigelow on the relation between solar magnetism and terrestrial temperature, and in the *American Meteorological Journal* for January on the subject at the head of this article. It may be of interest to readers of this journal if I make a few comments on the latter paper which is destined to attract world-wide attention. If the views therein set forth can be maintained and are accepted they will form an extraordinary departure from scientific methods thus far adopted by meteorologists and will mark an era in that science. So important is this discussion and so far reaching in its results that it should be put forth only after the most careful research and accurate substantiation of all the facts which enter it. It is safe to say that a score of meteorologists have been working in the same or similar lines, I mean in the effort at determining definite recurring weather conditions, and with very meagre and disappointing results thus far.

Professor Bigelow, in choosing these hurricanes as the basis for the investigation, has been particularly fortunate for the reason that these storms are quite well defined in their characteristics, occur in a rather circumscribed region, in a well defined period of the year, and can be easily and accurately collated. The only serious difficulty that can arise will be in determining the exact date of their origin, since it is well known that their beginning is in a disturbed condition often lasting for several days. We can avoid error in this regard by taking the day when violent or increasing winds first manifest themselves. It is very essential, however, in all meteorological inquiries of this sort, that we have a very large number of cases to work upon, and I think every one will be painfully impressed with the meagerness of the data upon which such a remarkable conclusion or law was based. I find only forty cases of hurricanes occurring during the years studied and these must be distributed over a period of about 27 days.

On examining the curves showing this magnetic influence upon these storms we see a seeming wonderful similarity amounting to well nigh an exact accordance between their crests and hollows. It would seem as though we would be abundantly justified in concluding that this influence is so marked we ought to find it at every recurrence of the dates in question, that is,

whenever the 6th, 17th, 22d and 26th days of the period came around (these are the more prominent coinciding dates selected by Professor Bigelow) we would expect to find a storm or at least a disturbance in this region. In the 19 years covered by this discussion, these were on the average, 14 recurrences of these dates or of this supposed influence, during each year, or 266 in all. The total number of effects or storms as given by the table, however, is only 28 on these dates, that is, out of a possible 266 cases there are only 28 successes, or 9.5 per cent which favor the influence idea. At the outset this would seem absolutely fatal to the supposition that there is any physical connection between these phenomena. We seem to have nothing more here then a mere coincidence in the few cases given. This position is all the stronger the closer the agreement in the few cases we have.

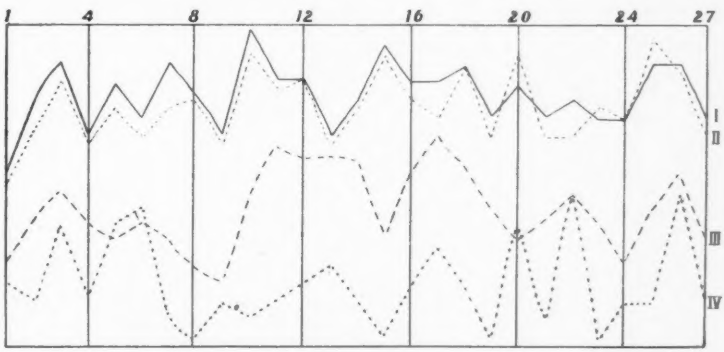
The marked discrepancy at the 20th day in the supposed law of coincidence, is most serious and cannot be lightly passed over. If it is found impossible to explain the occurrence of such a large number of hurricanes on an off day, it will throw a cloud upon the whole research, and this cloud will be all the denser the closer the agreement in the remaining cases. It will be voted that I have laid down general principles only which should be followed in studies of this kind. It seems to me there is a most serious defect in the fundamental magnetic influence curve. Is it possible to conceive of such well defined and such frequent changes in the Sun's magnetism or magnetic influence? If the Sun's magnetism undergoes such changes once in about four days and if the amount of this change has any marked value from the crest to the hollow, we certainly could not find any definite terrestrial effect following from it, for the terrestrial effects would be so blended as to mask entirely the original influence.

On examining more closely the material used, we are much surprised to find that 42 out of the 80 cases are of north north Atlantic storms, or storms above 45° north latitude, and which have no relation whatever to the tropical hurricanes we are supposed to be studying. It would give rise to great confusion in meterorology if one should undertake to study such definite phenomena as these tropical disturbances and at the same time mingle with them storms of a decidedly different aspect. The north north Atlantic storms cover a vast region; their place of origin is frequently far inland and impossible to ascertain; their aspects are often very difficult to determine; in every way they are unsuited to connect with the other storms and cannot be employed properly. I do not mean to say that these storms can-

not be used in proving a solar magnetic influence at the earth, for it is possible they may be so used, but the principles upon which they may be studied, and the rules to be adopted in collating them are so different from those for the others that they must be taken by themselves, or else it must be shown that the origin of the two classes of storms is under similar conditions and that there is a reasonable certainty of a strict comparison between the date, place and manner of origin, etc., of the two.

In the course of an investigation upon the storms of the western Gulf for the forecast division I have had occasion to prepare a list of the 144 tropical storms that have been observed in the waters about the West Indies and the Gulf of Mexico. In order to get some idea of their intensity I have at the same time placed a figure upon a scale of 1 to 3 against each storm, that is, 1 indicates a relatively weak, and 3 a relatively severe storm. This is not a scale of violence alone but one taking in the general extent of the storm and other characteristics showing intensity. The accompanying table gives these storms. The horizontal row beginning with (D) gives the date, and that with (I) the intensity.

I have distributed these storms by the days of solar rotation or magnetic influence adopted by Prof. Bigelow and the summary of these is here given, also the curves showing



I. Hurricanes by number of cases.
II. Same by weight or intensity.
III. Curve of solar magnetic influence.
IV. Hurricane curve found by Professor Bigelow.

These curves show facts as follows:

1st. There is a marked correspondence between the two top curves, and this was to be expected because they are of identical

Storms in the neighborhood of the West Indies during August, September and October, from 1874 to 1893.

	August.				September.					October.								
1874, D.					2	8	75											
I.					3	1	3											
1875, D.	1	6	13		9	24				13								
I.	1	1	1		3	1				2								
1876, D.					12					19								
I.					2					3								
1877, D.	2				14	17	21			16	24							
I.	1				3	2	3			1	1							
1878, D.	12	24			1	12	24	29		9	13	18						
I.	2	1			3	1	2	1		2	1	3						
1879, D.	13	16	20	30	8	12				3	10	25						
I.	1	3	1	2	1	1				1	2	2						
1880, D.	5	12	15	24	7					1	5	7	20	27				
I.	2	2	2	3	1					1	2	1	2	2				
1881, D.	1	16	22	27	9	14												
I.	1	1	3	2	1	1												
1882, D.	29				2	22				6								
I.	1				3	1				3								
1883, D.	15	23			4					8	22							
I.	3	2			3					1	2							
1884, D.					3	10				7	11	21						
I.					2	2				2	2	1						
1885, D.	8	23	30		15	18	24	26		10								
I.	2	2	1		1	1	2	2		2								
1886, D.	6	13	15	19	22	24				1	7	22						
I.	1	2	3	3	2	1				2	3	2						
1887, D.	5	15	19	30	1	11	15	24		6	8	9	11	14	16	22	28	29
I.	1	3	3	2	1	2	1	1		1	1	2	2	1	3	1	2	2
1888, D.	16	31			7	24				10	24							
I.	2	3			1	1				1	1							
1889, D.	8	20	25		1	4	13			1	4							
I.	1	1	1		3	1	3			2	2							
1890 D.	11	23	27		4					2	22							
I.	1	1	3		1					1	1							
1891, D.	18	26			6	11	14	18	25	1	6	13						
I.	3	1			2	1	1	2	1	1	3	2						
1892, D.	16				11					1	6	13	22	28				
I.	3				2					1	1	2	3	3	1			
1893, D.	15	20	22	29	1	5				1	21							
I.	2	2	3	1	1	2				3	1							

SUMMARY.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	15	26	27*
1	5	7	2	6	4	7	5	3	9	6	6	3	5	8	6	6	7	4	6	4	5	4	4	7	7	4†
0.5	3.5	6.0	2.5	4.5	3.0	4.5	5.0	2.5	7.5	5.5	6.0	2.5	4.5	7.5	5.0	4.0	7.0	3.0	7.5	3.0	3.0	4.5	4.0	8.5	6.5	3.0‡
1.3	3.5	5.1	3.4	2.5	3.3	2.6	1.0	0.3	5.1	7.6	6.9	6.9	6.6	2.7	6.1	8.0	6.5	4.1	2.3	3.4	4.9	3.3	1.1	4.2	6.0	2.7§
3	2	6	2	6	7	1	0	2	1	2	3	4	2	0	3	5	3	0	6	1	8	0	2	2	8	2‖

* Days of magnetic influence. † I Number of hurricanes. ‡ II Hurricanes by weight.
§ III Magnetic influence by Professor Bigelow.
‖ IV Hurricanes by number, Professor Bigelow.

phenomena. A larger number of storms would have brought about a slightly closer agreement between the seeming waves in these curves.

2. Any one at all familiar with work of this kind will at once see the utter hopelessness of comparing either of these curves, I and II, with that for magnetic influence III.

3d. The make up of the last curve IV. from such diverse records and from so few storms would have led us to expect very slight agreement with I or II, but one could have hardly anticipated such marked contradictions. It should be noted that I and II are rigidly constructed from the data gathered from the original records and collated too with absolutely no reference to a magnetic or any other period.

Some one may think that I and II would have been materially different if every slight disturbance in the West Indies had been collated. This is not true, however, for the reason that any farther disturbances, if there had been such, though a careful search did not show them, would have been distributed uniformly along the 26.68 days and would have simply given a horizontal rise in the curve all along the line and would not have changed the phases of the curve, or rather, its crests and hollows, in magnitude or position in the least. This is shown quite clearly by the marked correspondence between I and II.

It should be remarked, in closing, that I believe fully in a strong influence from the Sun's electricity or magnetism upon our weather and that too directly and not through its heat influence. I have been working at this problem for thirteen years and have obtained a few interesting results. I have also studied such curves as these by the thousand and have found the investigation practically hopeless without there is first an elimination of a variety of conditions or effects which serve to mask or cloud the correspondence between phenomena. Unless we can show how or why a physical connection can or may exist between diverse conditions, we are standing on most dangerous ground and are practically on precisely the same footing as the "planetarians," "lunarians," "cyclists," "sunspotists," etc.

Jan. 15, 1894.

Dr. A. Auwers has published, in *Astr. Nach.* No. 3195–96, the results of an exhaustive piece of work in the way of comparison of star-catalogues. He has compared the fundamental catalogue used in forming the 'Astronomische Gesellschaft' catalogues with forty-seven different others. The result occupies many pages of figures, and it is difficult to comment. The paper should be studied by those interested in star-catalogues.—*Observatory*, Jan., 1894.

FREE PUBLIC OBSERVATORIES.

W. W. PAYNE.

A movement of interest is noticeable of late that has for its object the founding of what may be called free public Observatories for instruction and entertainment in cities and large towns. The idea is by no means, a new one in this or foreign countries; for to a very limited extent it has prevailed, in localities where astronomy and its kindred branches have been cultivated, in one form or another, for many years. Very naturally thoughtful people, young or old, educated or uneducated, desire to gain some knowledge of the heavens whose wonders and awe-inspiring sublimity are spread out to human eye everywhere almost every day. Is it a wonder that Astronomical Observatories should unformly have the custom of setting apart one or more evenings each week to meet this common want in the people's mind? Is it a wonder that men of means who have some idea of the value of science in general should give largely to build and endow Astronomical Observatories and chemical and physical laboratories, when they know from common observation what factors such institutions are in the advancement of knowledge and the elevation of common thought and common life?

But our purpose now is not so much to call attention to the beneficent influence of science as a means of popular culture or elevating entertainment, as it is to notice the strong and pronounced phase of this influence in asking that measures be adopted speedily whereby it may be more effective and more generally useful. All agree that more should be done for the masses of our intense American life. But thinking men or philanthropists have not put themselves to the task, as yet, very severely, of working out the methods by which these worthy ends may be accomplished. It is true that College extension work and Chautauqua Circles have grown prodigiously within the last five years, and the demands are increasing. Books and apparatus have been adapted to this new order of things, and the change for the better in public sentiment in regard to the value of popular education is everywhere apparent. The walls of college caste even are yielding before this powerful wave and their cloister-like exclusiveness will soon deservedly be a thing of the past. Ruts in science, literature, politics or anything else mean stagnation and that is the sure symbol of death to any cause. Last of all it is

sad to admit it, but it is nevertheless generally true that astronomers have been ill-disposed, averse to, and coldly critical towards anything looking to a popular study of astronomy, unless such instruction be very sacredly guarded by appropriate technical language which involves the essentials of the higher mathematics that specialists only know how to use. This so-called learning is a delusion and a snare. For a scholar to say that he can not express himself concerning anything he knows in plain English so as to be understood by any one possessing common intelligence is a confession he ought to be ashamed of. The difficulty felt is not in the facts of astronomy or the nature of the science, as exacting as it is, but rather in the scholar himself. If he had a little more patience and less shameless pride he would be able to do much more for the cause of popular advancement in useful elementary astronomy. We would not now feel like speaking quite so plainiy if we had not seen unmistakable evidences of this thing within the last few months. On the other hand very strong appeals have come from persons interested in the study of popular astronomy recently and simultaneously in the great cities of Boston and New York, saying that the time has come when there should be built and equipped and endowed in each of these places free public Observatories for the purposes of instruction and elevating entertainment. In the February issue of *Popular Astronomy* and other scientific publications as well, will be found articles and notes indicating the sources and extent of the information referred to. It may here be added that the movement has been sufficiently prominent to claim the attention of the Boston Scientific Society at one of its late regular meetings, at which a paper on the theme was read by one of its prominent members which was followed by a general discussion showing general favor for, and hearty sympathy with, the idea of establishing a free public Astronomical Observatory for the city of Boston. It was argued that such an institution should be founded, it should be endowed, it should be wisely managed, and one of its objects should be the entertainment of the public. A scholarly astronomer, an authority in Europe and America in the most intricate and difficult questions of practical and theoretical astronomy, now advising that a free public Astronomical Observatory should be built for the entertainment of the public as one object! Why, think of it; are the scholars coming into line? Unmistakeably they are. And when they do move unitedly nothing probably less than a confusion of their mother tongue will stop them. Unquestionably this new impulse

is one setting in the right direction, and there is evidence that it will grow in strength because of the character of the people that are behind it. That it is wise and right and timely seems equally clear. The Boston beginning is a noble one and we hope it will gather strength and enthusiasm rapidly. The scheme in New York is less developed so far as known, but it is believed that New York will not be behind Boston in any worthy public enterprise of this kind. New York may possibly take the lead.

Death of Dr. Rudolf Wolf.—Astronomy has lost an earnest devotee by the death of Dr. Wolf, Director of the Zurich Observatory, who died on December 6 last, at the age of 77 years. Dr. Wolf was born in 1816 near Zurich, and his early studies were under the direction of Horner, Encke, and Poggendorf. In 1847 he was appointed Director of the Observatory at Berne, where he commenced his studies and observations of sun-spots, which studies led him to announce the connection between sun-spots and the Earth's magnetism. In 1855 he was appointed to the new Observatory at Zurich, which position he held at the time of his death. Dr. Wolf will probably be chiefly remembered for his work on sun-spots, in which he has been engaged for almost the last fifty years. His "Sun-spot Numbers," published annually, which give the state of the solar energy for each month in the year, as indicated by the total area of the spots on the Sun for each month in the year, have been of the greatest service to many investigators in this branch of science. As a member of the Federal Geodetic Commission, of which he was formerly President, Dr. Wolf showed an extensive knowledge of Geodesy, and contributed 'A History of Geodetic Measures in Switzerland' to its literature. Among his other works are a 'History of Astronomy,' which is a most valuable work of its kind, and a 'Treatise on Astronomy,' which is his latest work. Dr. Hirsch, Secretary of the Geodetic Association, thus writes of him:—

"During his long life, wholly devoted to science and filled with sustained and fruitful work, our friend preserved to the last days of his short illness the serene tranquility which marked his lovable character. His fine figure will remain engraven in the memory of his friends and colleagues as the very type of an indefatigable and powerful worker in the great field of science."—*Observatory*, Jan., 1894.

We regret also to have to record the death of Dr. Adolphe Steinheil, chief of the optical firm of Steinheil & Sons, Munich, who died November 4, 1893, aged 61. Also of Friedrich Gustav von Bülow, who was the founder of the Observatory at Bothkamp, from which MM. Vogel and Lohse issued the three volumes of the Bothkamp observations, 1870–74.—*Observatory*, Jan., 1894.

Astro-Physics

THE SOLAR FACULÆ.*

GEORGE E. HALE.

The almost simultaneous discovery of the doubly reversed H and K lines in the spectrum of the faculæ, made by M. Deslandres and myself in 1891, was not altogether unexpected. Several years earlier, Professor Young had seen this bright pair of lines in the spectrum of spots, but their position at the extreme limit of the visible spectrum made satisfactory observation impossible. Professor Young found, however, that the bright lines were not confined to the spot itself, but extended on to the disc for a considerable distance. With the application of photography the difficulties of visual observation disappeared, and the bright lines were found, not only in the neighborhood of spots, but also in extensive regions irregularly distributed over the solar disc. A dark central line of double reversal, which had escaped the eye of the observer, was also clearly registered upon the photographic plate. With a sufficient dispersion it is occasionally found that the doubly-reversed lines extend entirely across the Sun. They are not uniformly bright, but have alternate maxima and minima of intensity. In the minima the lines sometimes seem to disappear completely, but it appears probable that with sufficient dispersion they could be photographed at any point on the disc. H and K are almost identical in appearance, except that the latter is always the brighter cf the two.

The question has been raised as to whether the bright lines with dark centres are true double reversals, or simply close double lines. A moment's consideration of the facts of the case ought to leave no room for doubt on this point. If a photograph of the spectrum is taken with the slit lying across the Sun's limb, it is found that the two bright lines of the double reversal unite into a single bright line at the edge of the disc, and this single bright line exactly coincides in position with the central dark line of the double reversal. This is shown in the photograph from which the cut was made, but the reproduction fails to bring it out.

The investigations of Cornu and other physicists, on the reversal of the lines of metallic vapors in the electric arc, offer most striking analogies to the phenomena just described. Through

* Communicated by the author.

the kindness of M. Cornu I have recently had the pleasure of examining some of his photographs of reversed lines. The investigations were confined to the ultra-violet, and the calcium lines which correspond with H and K were not specially studied. The reversals of other lines are similar, however, and will serve our present purpose equally well. An ultra-violet line of aluminium on one of the photographs reproduces the solar H and K reversals so perfectly that it might readily be mistaken for one of them were there any doubt as to its mode of production.

In M. Cornu's experiments an image of the arc was formed on the slit of the spectroscope. A portion of the metal, or one of its salts, was introduced into a cup-shaped cavity in the lower carbon, and vaporized by the passage of the current. Under these conditions the central portion of the arc, where the line of sight passed through the cool exterior vapor to the intensely heated vapor and the glowing carbon poles, showed the aluminium line reversed—two narrow bright lines enclosing a narrow dark line. At the edge of the arc, however, where the line of sight passed through only the cooler vapor of the exterior, the two bright lines united into a single bright line, corresponding in position with the dark line seen in the first case.

We thus arrive at a basis for the interpretation of the H and K reversals in the Sun. In the arc we have a reversed line produced by the absorption of the cooler vapor of the exterior. The chromosphere would seem to play the part of the absorbing vapor in the Sun. At the base of the chromosphere, or below it, is the hotter vapor corresponding with that at the centre of the arc. Here is the seat of the brilliant radiation of calcium, which produces the two bright components of the doubly-reversed H and K lines. The upper part of the chromosphere, on the other hand, acts as an absorbing screen, and produces the central dark line of the reversal. A photograph of the solar disc secured by means of the spectro-heliograph (using the K line), should therefore show not the entire chromosphere, but only its lower and hotter parts.

The distinction is an important one when it is remembered that photographs taken with the spectro-heliograph show the entire surface of the Sun to be mottled over with small, irregularly shaped, bright regions, which seem to form a nearly unbroken reticulation (ASTRONOMY AND ASTRO-PHYSICS, May, 1893, p. 450). This is not to be mistaken for the well-known "granulation" or the "réseau photosphérique" of M. Janssen. Neither does it seem at all probable that the brighter regions are merely elevations in the chromosphere, for if the upper part of the chromo-

sphere acts as an absorbing medium, and produces the dark central line of the H and K reversals, an increase in the depth of the chromosphere would certainly not diminish the absorption. It seems likely that the reticulation represents a true facular network, for the small faculæ seen without the spectroscope near the Sun's limb, and well described by Secchi (*Le Soleil*, German edition), are probably parts of the same reticulation. Probably no sharp distinction can be made between the base of the chromosphere and the upper surface of the underlying faculæ. The latter seem to be in intimate connection with the interior of the Sun, and one might therefore expect to find the H and K lines very bright in them.

In this connection it should be remarked that the H and K lines over spots are frequently somewhat narrower and less brilliant than on the disc, and the central dark line is often absent. I have explained this as probably due to the fact that we are here dealing with the radiation of the chromosphere overlying the *cooler* region of the spot. (ASTRONOMY AND ASTRO-PHYSICS, 1892, p. 815.)

In a recent paper on the "Physical Constitution of the Sun" (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 832), Father Sidgreaves has expressed his belief that faculæ are prominences seen in projection on the solar disc, and M. Deslandres has advocated a similar hypothesis in the December number of *Knowledge* (see also *Comptes rendus*, Nov. 27, 1893). As my own position in regard to the subject is evidently not fully apprehended by M. Deslandres, I shall endeavor in what follows to state it as clearly as possible.

In a note dated January 18th, 1892 (ASTRONOMY AND ASTRO-PHYSICS, February, 1892, p. 159), I wrote as follows in regard to the regions on the Sun's surface in which I had found the H and K lines to be doubly reversed: "On January 12th, 1892, it was found possible to photograph the *forms* of some of these reversed regions, using a moving slit apparatus just completed for our large diffraction spectroscope by Brashear. The K line in the fourth order spectrum was employed, as is customary in the case of prominences. The reversed regions are of great extent, and in appearance closely resemble faculæ. Several explanations may be suggested to account for them. They may be:—

"1. Ordinary prominences projected on the disc.

"2. Prominences in which H and K are bright, while the hydrogen lines are absent.

"3. Faculæ.

"4. Phenomena of a new class, similar to faculæ, but showing only H and K bright, and not obtained in eye observations or ordinary photographs because of the brilliant background upon which they are projected."

Subsequently I found, by comparing photographs of faculæ near the Sun's limb, made at the focus of a telescope in the ordinary manner, with photographs of the reversed regions made with the spectro-heliograph, that there was a very close agreement in form. For this reason I adopted the provisional name "faculæ" in subsequent references to the bright regions shown on spectro-heliograms, reserving an exhaustive discussion of the phenomena until sufficient material had been collected for that purpose. Part of this material, in the form of about three thousand spectro-heliograms and several hundred ordinary photographs of the Sun, had already been collected and partially reduced. It is intended that one of the first volumes of publications to be issued by the Yerkes Observatory shall be devoted to a discussion of these results. At the present time, and at a distance from my photographic and other records, I can discuss the subject only in a provisional way.

No one can doubt that prominences, and particularly eruptive prominences, are closely related to faculæ. To this point I have already called attention in the following words: " . . . In a great many photographs taken with the spectro-heliograph, faculæ are shown projecting above the Sun's limb. And the intimate relationship between faculæ and eruptive prominences is not less evident, especially in composite photographs showing faculæ and prominences on the same plate. When we consider that *eruptive* prominences probably rise from faculæ, it is not at all surprising that such prominences sometimes show a continuous spectrum in addition to their bright lines. For a violent eruption would naturally carry up with the prominence some "dust-like" matter from the facula, which would give a continuous spectrum." (ASTRONOMY AND ASTRO-PHYSICS, November, 1892, p. 815).

The projection of faculæ at the limb, while very frequently shown in short exposure photographs* of the Sun's disc, is rarely much greater than the average depth of the chromosphere. If faculæ are prominences seen in projection, or if they are always covered by prominences, as Father Sidgreaves and M. Deslandres hold, these projections should be much higher—*i. e.*, we should

* Obtained with the spectro-heliograph and also by direct exposure at the focus of a telescope.

always find a prominence above one of these projecting faculæ. As a matter of fact, the long exposure spectro-heliograms of the chromosphere rarely show prominences at such points; when prominences are present they are almost invariably eruptive, and of small extent at the base. But the projecting faculæ give the reversed H and K lines, even when no prominence is present. M. Deslandres states, however: "Les facules sont, par définition, les plages brillantes de la surface solaire, plages qui, à l'intensité générale près, donnent les mêmes raies noires que les parties voisines, et correspondent aux parties élevées, aux montagnes de la photosphère. Elles sont distinctes des flammes de calcium audessus d'elles." (*Knowledge*, December, 1893, p. 230).

It is probably true that the faculæ are ordinarily quite different from the prominences which sometimes cover them, but I cannot see that any evidence, other than a "definition," is offered to prove the absence of the bright H and K lines from their spectra. These lines may have their origin in hot calcium vapor distributed through the mass of the facula, or confined to its outer portion,* but I by no means consider it proved that the spectra of faculæ do not contain the bright H and K lines.

Leaving for a moment the question of faculæ, let us next consider whether prominences projected on the solar disc should be rendered visible by the spectro-heliograph. The reasoning which has already led us to the conclusion that the nearly continuous reversals of the H and K lines on the disc originate at the base of the chromosphere would seem to apply with greater force to the base of prominences, for here the H and K lines are apparently brighter than in the surrounding chromosphere. And, in fact, I have previously shown that certain outbursts on the solar disc, which, there is every reason to believe, are true eruptive prominences, have been photographed at the Kenwood Observatory with the spectro-heliograph (see ASTRONOMY AND ASTRO-PHYSICS, 1892, p. 611; *ibid*, p. 920, Plate xlvi., photographs of the eruptive prominence of July 15, 1892; *ibid*, 1893, p. 454). Such brilliant outbursts are quite exceptional, only seven having been found in over two thousand spectro-heliograms.

While it is thus certain that some promineuces can be detected on the Sun's disc, it is equally certain that others cannot. Since

* It may even be that the lines originate in the chromosphere overlying the faculæ. In this case, the increased brightness of the lines, as compared with their brightness in other parts of the chromosphere, would have to be accounted for. Any such brightening, if due to the faculæ, would in all probability be confined to those parts of the chromosphere immediately overlying the faculæ, so that the forms of the latter would still be obtained in spectro-heliograms.

January, 1892, bright regions in which the H and K lines are reversed have been found only in the sunspot zones. Not having access to our records, I cannot give the northern and southern boundaries of this facular zone with accuracy, but I do not think we have photographed a single bright calcium region more than 70° north or south of the equator. During the same period our photographs have shown great numbers of prominences of higher latitude than 70°, and prominences have not been uncommon in the near vicinity of the poles. Thus there are bright prominences which give no indication of their presence when projected on the disc, for by no flight of the imagination could the small and evenly distributed meshes of the facular reticulation be supposed to represent the bases of such large and brilliant prominences. In the face of this difficulty, I prefer to wait for further evidence before adopting the conclusion that quiescent prominences in the sunspot zones can be photographed when projected on the disc.

M. Deslandres has suggested that the reversals of calcium on the solar disc be called "*flammes faculaires*, nom qui est en accord avec les faits, et evite toute ambiguité." (*Knowledge*, December, 1893, p. 231; *Comptes rendus*, Nov. 27, 1893). I regret that, for the following reasons, I cannot consistently adopt this name:—

1. The use of the identical term "flame," commonly employed to describe one variety of chemical combination, is objectionable, because we do not know that the solar and terrestrial phenomena referred to are in any way similar.

2. I have offered evidence to show that many faculæ are not covered by prominences, but themselves give the H and K lines.

3. Even if it could be shown that all faculæ are covered by prominences, it would seem unnecessary to replace the well-known term "prominence" by a less satisfactory synonym.

For the present, if one does not wish to commit himself by speaking of faculæ and prominences on the disc, the general term "calcium reversals" may perhaps be used, though it is not altogether free from objection.

As to the electric origin of the bright H and K lines in the Sun, (Deslandres, *loc. cit.*) it seems to me that the merely negative evidence at our disposal is not a safe foundation on which to build an argument. It is true that the hydrogen spectrum has not hitherto been obtained in the laboratory by the simple effect of heat, but it does not follow that this gas would not give a spectrum of bright lines when subjected to solar conditions. The H

and K lines of calcium had not been obtained artificially without electrical means until I succeeded in photographing their feeble radiations in certain flames (see ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 452). At solar temperatures these radiations may be greatly strengthened, and the gradual shift toward the violet of the maximum of intensity in the calcium spectrum noticed with increased temperatures renders such a strengthening probable.

While it seems quite possible, and even probable, that electricity plays some part in solar phenomena, the evidence upon which to base any very positive statements appears to be lacking.

Before passing on to the discussion of certain practical questions, important in connection with future investigations of the Sun, let us sum up some of the conclusions to which we have been led.

The faculæ are the elevated regions of the photosphere. They form an irregular reticulation over the entire surface of the Sun,* and in the sunspot zones appear as irregular bright regions of varying extent. The spectrum of the faculæ is similar to the general spectrum of the Sun, but is somewhat brighter, and contains the doubly-reversed calcium lines H and K.† The hot calcium vapor from which these lines emanate may be diffused throughout the mass of a facula, or confined to its upper surface. In the latter case, the base of the chromosphere and the upper surface of the facula would practically coincide. The white-hot particles giving a continuous spectrum would ordinarily be found in the facula proper, and (more sparsely scattered) in the lower region of the chromosphere. *Eruptive* prominences are closely related to faculæ, and probably rise from them. It thus occasionally happens that a violent eruption carries some of the white-hot particles to a considerable distance above the photosphere. In such a case the prominence gives a continuous spectrum in addition to its bright lines. While some faculæ are covered by prominences, others do not appear to be so covered. Certain exceptionally bright eruptive prominences have been photographed in projection on the solar disc. Ordinary prominences in the region of the sun's poles are not shown in spectro-heliograms. Sunspots, even in their central parts, seem to be covered by the chromosphere (or by overhanging prominences). The chromosphere (or prominences) is frequently so bright as to completely hide small spots in spectro-heliograms.

* At least during the maximum period of sunspots.

† The less refrangible hydrogen lines may also be present, but under ordinary conditions they are too faint to be recognized.

As M. Deslandres has discussed the instruments and methods employed in my photographic investigations of the Sun, I may perhaps be allowed to express an opinion as to the most advantageous manner of continuing these researches.

The spectro-heliograph used in the greater part of my photographic work has a pair of slits arranged to move in the focal planes of the collimator and observing telescope of a large diffraction spectroscope, attached to a 12-inch equatorial refractor.* It has proved itself a thoroughly practical instrument, and from two to twenty or more photographs of the forms of prominences and calcium reversals have been made with it on every clear day (with few exceptions) since January, 1892. Neither my assistant, Mr. Ellerman, nor myself have experienced any difficulty in using a second slit 0.005 inch wide, and this width could be decreased were it considered desirable. By increasing the width of the second slit and making a series of photographs of the K line at various points on the solar disc with the slit stationary, the character of the double reversals can be studied. This method of successsive sections, which I first employed in 1891, seems to me quite as convenient as that used by M. Deslandres. As the slits always move together there are no troublesome adjustments to be made by hand.

But after studying and experimenting with a great variety of instruments, I came to the conclusion early in the year 1893 that a spectroscope with collimator and observing telescope parallel (or nearly so) to each other, and slits fixed in the axis of each, the whole instrument being arranged to move on wheels at right angles to the axis of the large telescope, would possess important advantages over all other forms of spectro-heliograph. An instrument of this type, suitable for use with a heliostat for automatically photographing the Sun, was described in my paper entitled "*The Spectro-heliograph*" (ASTRONOMY AND ASTRO-PHYSICS, March, 1893, p. 256). I have since designed a spectro-heliograph on this principle for the 40-inch Yerkes telescope. A short description of this instrument will be found in the January (1894) number of ASTRONOMY AND ASTRO-PHYSICS. The proposed "rotating spectroscope," for which M. Deslandres' various papers have claimed many advantages, has, I believe, been recently abandoned for a spectro-heliograph of this class. A similar instrument is also to be used at the Maharajah Takhtasingji Observatory at Poona, India.

* This telescope will be removed to the Yerkes Observatory, where the spectro-heliograph will be employed, as at present, in securing a daily record of solar phenomena.

The question raised by M. Deslandres, in regard to the best dispersion to employ, is a most interesting and important one. On the one hand, a feeble dispersion would seem to offer important advantages on account of the narrowness of the K line and the greater brightness of the image. On the other hand, it must not be forgotten that the fainter details of the reversals may be lost if the dispersion is insufficient. M. Deslandres recognizes this fact when he states that the exceedingly faint H and K reversals in the general spectrum of the Sun are best obtained with a "spectroscope puissant" (*Knowledge*, December, 1893, p. 231); and again, when he remarks that for the study of the details of the reversals on the solar disc "une grande dispersion est necessaire" (*Ibid*, p. 232). This advantage of high dispersion is due to the fact that the width of the bright lines is not proportional to the dispersion, at least up to a certain limit. If by doubling the dispersion the lines were doubled in width, there would evidently be no change in their brightness as compared with the solar spectrum in which they lie. But, within certain limits, the brightness of the lines with respect to the solar spectrum increases with the dispersion. It is evident, therefore, that we must not employ too feeble a dispersion. I have found the fourth order spectrum of a Rowland grating (14,438 lines to the inch) very suitable, though I should have preferred prisms had circumstances permitted their use. The separation of the H and K lines at the focus of the observing telescope, where the photographic plate is placed, is nine millimètres. M. Deslandres uses a single prism, giving a separation of H and K of two millimetres, and magnifies the image of the second slit—and, consequently, the width of the K line—three diameters. The resulting separation of H and K on his photographic plate would thus be six millimetres, and the width of the K line two-thirds the width of the K line in my instrument, if the line were supposed to have a width proportional to the dispersion. As has already been pointed out, the width of a line is not proportional to the dispersion, and it is probable that there is no great difference in the effective width of the line in the two instruments. Thus, any question in regard to displacement due to motion in the line of sight, width of the second slit, etc., would apply almost equally in both cases; but the greater dispersion of the grating would increase the brightness of the K line as compared with the solar spectrum, and fainter and more delicate calcium reversals should be obtained by its means.

It would probably be advisable to have a set of three prisms

for a spectro-heliograph, so that one, two, or three might be used as occasion required. The method of forming a magnified image of the second slit on the photographic plate, which we owe to Dr. C. Braun, formerly Director of the Haynald Observatory at Kalocsa, is in some respects a valuable one. Its principal defect—the widening of the K line and the second slit—can be avoided by enlarging the solar image before it enters the spectro-heliograph.

The large solar spectroscope which is to be used with the Yerkes telescope will be specially arranged for the study of the H and K reversals on the solar disc. Among the attachments to be employed for this purpose will be a pair of long slits, arranged to move in the focal planes of the collimator and observing telescope. The method of photographing the K reversal in successive sections of the disc will thus be similar to that hitherto employed with the Kenwood Observatory spectro-heliograph, but the exposures will be made automatically by an electrical device controlled by an astronomical clock. As I have already remarked, the spectro-heliograph for the Yerkes telescope will be arranged on another plan.

BERLIN, Dec. 19, 1893.

ON TWO GREAT PROTUBERANCES.*

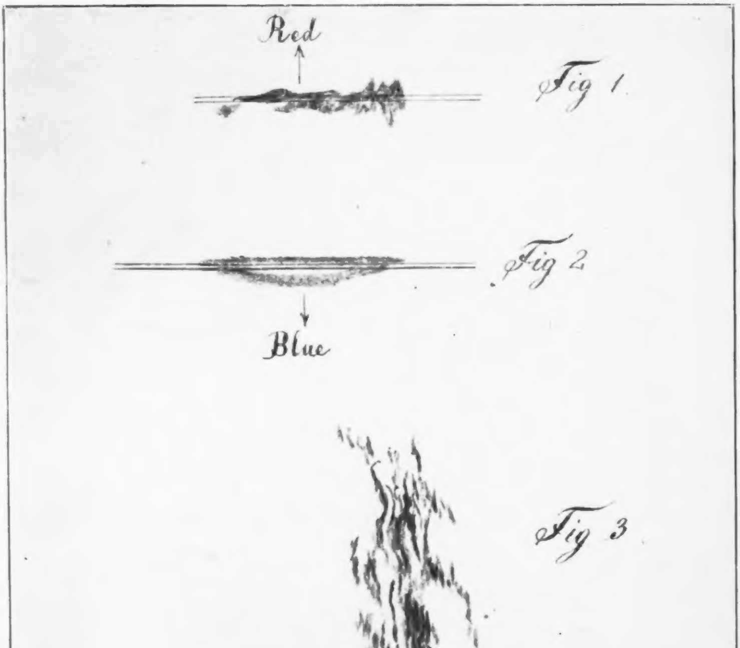
J. FENYI, S. J.

On the 19th and 20th of September, 1893, I had an opportunity of observing two protuberances which, on account of their enormous size and motion and their occurrence within the same twenty-four hours, are of great interest in the theory of these phenomena.

The first one was found on Sept. 19, 2 P. M., Greenwich mean time, as a bright protuberance on the west limb of the Sun, in position angle 271° 14′ to 278° 0′, *i. e.*, in — 17° 0′ to — 23° 26′ heliographic latitude. The entire mass already betrayed a large motion in the line of sight. At the lower part, at 278°, the spectral light spread out toward the red; but the entire remaining mass showed a much more rapid motion toward us, by a displacement in the direction of the blue end of the spectrum (Fig. 1). At $2^h\ 12^m$ I measured this displacement with a filar microm-

* Translated from the original communicated by the author. German geographical miles have been reduced to English miles.

PLATE VI.



PROMINENCE OBSERVED AT KALOCSA 2^h P. M. SEPT. 19, 1893.

eter, and found that the velocity was 297 kilometres per second. After completing a drawing (reproduced in Fig. 3) in which the lower parts are carefully drawn, but the upper somewhat hastily filled in, in order to represent the structure of the image, I began to measure the height of the protuberance by observing its passage across the slit. At the first transit I obtained a height of 368″.5, and not in conformity with the drawing, I found an interruption in the image, extending from the height of 175″ to 294″. Nine transits were observed, in which the seconds of time were counted continuously and the instants of passage noted. By this method not only were the intervals between transits exactly obtained, but also the absolute moments when the summit of the protuberance entered the slit, and the intervals of time between the separate measures of the investigation. The following table gives a view of the details of the rise of this protuberance. The times are given in mean time of Greenwich to the nearest tenth of a second. In computing the motions the figures noted in the observing book were used, and the necessary corrections were afterwards applied.

PROTUBERANCE OF SEPT. 19, 1893.

Greenwich Mean Time.	Duration of Transit.	Height of the Protuberance in seconds of arc.	Velocity of ascent in kilometers per second.	Computed Acceleration in meters per sec.
h m s	s	″		per sec.
2 21 5.4	24.7	368.5		
21 51.1	(23.6)	352.8	114.6	
22 37.1	25.6	383.2		+ 151
			126.1	
23 35.5	26 3	393.5		+ 1613
			214.0	
24 24.6	27.3	408.2		+ 3666
			406.0	
25 18.9	29.4	439.0		− 1204
			339.6	
26 15.1	31.2	465.7		− 3980
			103.9	
27 15.7	31.8	474.5		+ 2235
			240.1	
28 23.2	33.3	498.0		

In the computation 1″ at the limb of the Sun was taken to be 724 kilometres.

The protuberance therefore rose 129″.5 in 7^{m} $17^{s}.8$. From this we get for the mean velocity of ascent 212 kilometres or 132 English miles per second. The greatest observed height reached 8′ 18″, or 0.520 of the Sun's radius—in absolute measure 224,000 miles.

Before I measured the height, I endeavored to make a faithful

sketch of the form of the prominence, but in spite of all my care the drawing had little value, on account of the rapid change of the object; however, I had an opportunity during the work to observe the structure and the changes of form in some detail. Throughout the whole course of its appearance the entire object consisted simply of very bright luminous bands or strips scattered one after another in ragged forms, and apparently lying nearly at right angles to the limb of the Sun (Fig. 3). They were strikingly bright even in the highest parts of the prominence. The form as a whole was also like a band or stripe, which had no pronounced inclination, but stood erect nearly in the direction of the Sun's radius. The whole enormous structure had a large motion toward us. In the observations of transits already mentioned, the summit of the protuberance, when it came into view, was outside the image of the slit, and the displacement continued as far as the chromosphere. The amount of the displacement, which was subject to numerous fluctuations, was about the same as when I measured it in the beginning with the micrometer, and therefore represented a velocity toward us of about 300 kilometers per second. At 2^h 30^m nothing more was to be seen of the whole gigantic structure than a small protuberance, whose height, according to an estimate, was about 30″. The eruptive metallic line λ 6677 was seen bright in the chromosphere from 286° to 275°. A group of faculæ was going over the limb of the Sun.

Now it is very remarkable that although these eruptive phenomena were displayed with most unusual magnificence, so that even in this year of heightened solar activity they were then without a parallel, on the very next day there should occur a still mightier eruption at a place nearly opposite to that of the first. This time the whole occurrence, from beginning to end, was displayed before my eyes. It was a few minutes before 9^h on the 20th of September when I saw a singular bright form in position angle 115° 36′ — 112° 32′, which was produced by displaced spectral light, and was therefore outside the image of the slit (Fig. 2). Its unusually diffuse aspect, and especially the circumstance that there was no protuberance standing on that place, induced me to regard the appearance more attentively, and to measure its position. While I was doing this, and preparing to measure the considerable displacement towards the red, a very strong and unusually brilliant arch arose, which likewise showed a considerable displacement toward the red end of the spectrum. According to two measurements with the filar micrometer the displace-

ment corresponded exactly with a velocity of 255 kilometres per second.

In the mean time the circumstances had entirely changed. In the few minutes during which I was occupied in the manner already referred to, a mass of prominence-forms had arisen on the part of the limb between 102° and 118°, and in the middle, at 107° 16′ to 112° 0′, towered the brilliant prominence which is the subject of the following measurements. The rapidity of its development allowed no time to make a drawing, and a rough sketch which was made in the greatest haste is without value. It will perhaps be sufficient to remark, in this connection, that this protuberance was strikingly similar to the one already described, not only in its outline, but particularly in its band-like structure. The direction of the whole eruptive stream was likewise sensibly coincident with the Sun's radius. I now—at $9^h\ 7^m$—hastened to measure the height of the already enormous mass by observing its passage across the slit, in the manner described above. During the eight transits, each of which lasted from 37 to 60 seconds, I had time to include several conspicuous intermediate points in the measurements. The first transit gave a height of 8′ 6″ or 352,000 kilometres. As this height was attained in about 12 minutes, the mean velocity must have been 488 kilometres per second, a result which is in tolerable accordance with subsequent exact measurements. The following table shows the progress of events during the eight transits.

PROTUBERANCE OF SEPT. 20, 1893.

Greenwich Mean Time.	Duration of Transit.	Height of the Protuberance in seconds of arc.	Velocity of ascent in kilometers per second.	Computed Acceleration in meters per sec.
h m s	s	″		per sec.
9 8 13.4	36.8	486.0		
			286	
9 3.5	38.3	505.5		+ 2904
			437	
9 56.8	40.7	538.1		− 255
			423	
10 51.7	43.2	570.7		− 1320
			347	
11 50.9	45.4	599.4		− 2800
			171	
12 56.2	46.6	615.0		+ 4740
			498	
14 7.2	50.3	664.5		− 3280
			262	
15 18.3	52.3	690.6		

From this we see that the protuberance finally reached the enormous height of 11′ 30″.6, or 0.722 of the Sun's radius,—in absolute measure 500,000 kilometers. In an interval of $7^m\ 4^s.9$

the protuberance rose 204''.6, from which we obtain an undoubtedly correct mean velocity of 214 miles per second. The maximum observed veloticy was 309 miles.

This protuberance, like the first, showed throughout its entire height, (*i. e.*, throughout its passage across the slit), a displacement toward the red, which, according to two measurements, corresponded to a motion of from 150 to 159 miles in a direction away from the Earth.

After $9^h 15^m$ the sky became cloudy; but the dissolution of the prominence had already set in. The last two transits were already somewhat uncertain on account of the faintness of its highest parts.

The occurrence in the same 24 hours of two such vast and infrequent outbreaks in the gaseous envelope of the Sun is very surprising. In the course of my observations of the Sun's limb, which are made daily whenever possible, I have met with no similar occurrence in the whole of the present year (1893). In this period of increased solar activity protuberances of 70'' height are to be found every day, and those of 120'' are not infrequent. The greatest heights observed in this year, exclusive of those now under consideration, were 260'' on March 28, 215'' on June 29, and 294'' on Sept. 23. When therefore as in this case, heights of 498'' and 690'' are reached, and enormous motions are exhibited, by prominences which are separated in time by an interval of only 19 hours, we are led to suppose that these two extraordinary apparitions have some real though perhaps remote relationship. This view has support in the circumstance that the prominences were situated on nearly diametrically opposite parts of the Sun, and were similar in form, structure, and progress of development. Both appeared to consist simply of bright bands or strips, and seemed to shoot upwards fiery streams of matter which were not sensibly inclined to the vertical, (leaving out of consideration the motion in the line of sight), and which did not spread out at a great height; while eruptions observed in other years presented quite different forms and cources of development. Some emphasis might also be laid on the fact that both were very bright up to enormous heights. In the sketch of the first prominence it was noted that the brilliancy was extraordinary half way to the summit. On observing even the last transits on the 20th of September, the image was bright at a height of from 351'' to 637'' — only the upper part was faint. Metallic vapors, which were indicated in the base of the first protuberance by the presence of the eruptive

line λ 6677, were not noticed in the base of the second. At that place on the surface of the sun no luminous form could be recognized which could be considered as related to the eruption.

When we regard these appearances in their entirety and in detail, and seek to understand their nature, we may well find ourselves in doubt whether we can regard them as produced simply by the mechanical motion of matter; we feel driven to assume an apparent motion, having its origin in the rapid propagation of a physical or chemical process. The simplest representation of the appearances which have been described is offered in the theory of H. Brester, which regards the prominences as nothing more than the luminous condition of that part of the Sun's gaseous envelope where the dissociated elements of hydrogen are cooled down to the point of recombination. We must confess that not only do the enormous velocities observed in these prominences find a welcome explanation in this theory, but that the details of the development and the banded structure are brought into better accord. The cloud-like, tattered structure of protuberances in general, and especially the often-observed process of dissolution, which is almost perfectly like that of our own clouds, would be exactly what we should expect as a consequence of such local explosions or luminous outbreaks in the Sun's gaseous envelope. The theory has naturally many difficulties to encounter which I cannot enter into here. I may, however, be allowed to point out one argument that demonstrates the impossibility of regarding as successful any of the attempts yet made to explain the great motions of the prominences as the propagation of a luminous condition; and that is the undoubted fact of the displacement of spectral lines, which requires the assumption of motions as great as those which are directly observed in the rise of prominences. I regard it as impossible that a displacement of the spectral lines can be produced by the mere advance of a physical or chemical process without a progressive motion of the light emitting molecules which constitute the mass. By accepting such an explanation as this we should leave the firm ground of experience without having any support whatever from theory.

Before we can assume an explanation of this kind, it must be shown that in such progressive explosions the molecules have at least a temporary motion in the direction of propagation, corresponding in amount to the enormous velocities found by observation. Moreover, the direct proof that a displacement of the spectral lines takes place in explosions, in a manner determined by the direction of propagation, is not without the province of experi-

ment. If the explosion of our known gaseous combinations takes place at the rate of only a few kilometers per second, the velocity of hundreds of kilometers in the Sun may well be ascribed to the greater heat there or to the nature of the dissociated atoms; a displacement, even though a minute one, must therefore be demonstrable by experiment in our laboratories. The infrequent strongly disturbed prominences could then be regarded as explained. It would then be the turn of the common, quiescent prominences, which all day long may be seen floating high above the limb of the Sun, without undergoing any important changes.

Since the assumption of electrical forces only embarrasses explanation in a province which is still obscure, we must confess that the nature of the solar protuberances remains an open question.

ON A CERTAIN LAW IN THE SPECTRA OF SOME OF THE ELEMENTS.*

C. RUNGE.

The list of standard wave-lengths lately published by Rowland in ASTRONOMY AND ASTRO-PHYSICS includes several of those doublets and triplets of lines, whose width and distribution has been studied by Prof. Kayser and myself. It is of interest to inquire whether Rowland's most exact measurements confirm the law that the doublets or triplets of the same series give the same difference of oscillation-frequencies. In the following table I have collected all the instances and I only regret that they are not more numerous. The wave-lengths have not been reduced to vacuum, because it is unnecessary to do so. For in all the given cases the lines are so near one another that either the difference of oscillation frequencies is, by the reduction, altered much less than the probable error of the determination, or the differences are altered so nearly by the same amount, that the deviation is much less than the probable error.

The error in the determination of the wave-lengths may, according to Rowland, amount to $\frac{1}{100}$ Angström unit. From this the greatest possible error of the difference of oscillation frequencies is calculated. For the calculation of the mean difference the weight of each difference is taken inversely proportional to the square of the greatest possible error.

* Communicated by the author.

	λ	1/λ	Difference.	Weight.	Greatest possible error.	Deviation from mean.
Sodium	6160.970	16231210				
			17246	14	52	19
	6154.431	16248456				
	5896.154	16960208				
			17196	12	58	31
	5890.182	16977404				
	5688.434	17579531				
			17239	10	62	12
	5682.861	17596770				
		Mean:	17227			
Magnesium	5183.792	19290897				
			40728	7	74	11
	5172.871	19331625				
			20089	7	74	11
	5167.501	19321714				
	3838.430	26052318				
			40678	2	136	39
	3832.446	26092996				
			20039	2	136	39
	3829.505	26113035				
		Mean:	40717			
			20078			
Calcium	6162.383	16227489				
			105900	14	53	2
	6122.428	16333389				
			52288	14	54	28
	6102.891	16385677				
	3973.835	25164608				
			105913	3	127	11
	3957.180	25270521				
			52128	3	128	132
	3949.034	25322649				
		Mean:	105902			
			52260			
Aluminium	3961.680	25241817				
			112092	25	128	11
	3944.165	25353909				
	3092.962	32331467				
			112133	9	209	30
	3082.272	32443600				
		Mean:	112103			

The deviation from the mean value is in each case considerably smaller than the greatest possible error except for the difference between the second and third line of the second Calcium triplet.

But even here the deviation might be made smaller than the greatest possible error by giving the second triplet a greater weight. The weight 5.5 for instance instead of 3 makes the mean 52243 and the deviations from the mean value 45 and 115 both within the limits.

There is one more triplet of Calcium lines in Rowland's list, but it does not belong to the same series as the two given above. As Kayser and I have shown, the first two lines of the triplets that belong to the same series are a little nearer to one another. But the second and third lines give the same difference of oscillation frequencies in both series. Rowland has

λ	$1/\lambda$	Difference.
4435.852	22543584	52176
4425.609	22595760	

The deviation of the difference from the mean value 52243 is 67, while the greatest possible error is 102.

Rowland gives three more Aluminium doublets. They belong to the same series as the second one of the two given above. They ought properly to be called triplets, for the less refrangible line is double, the weaker component giving with the most refrangible line the constant difference of oscillation-frequencies. Unfortunately Rowland's list does not include the weaker component, except in the case mentioned above.

From the accordance of Rowland's determinations with the law of the constant difference of oscillation-frequencies, one may draw two conclusions; first, that this law is in all probability absolutely correct, and secondly, that Rowland has not overrated the exactness of his measurements.

ON THE MOTION OF ζ HERCULIS IN THE LINE OF SIGHT.*

A. BELOPOLSKY.

I desire to call the attention of spectroscopists to the star Herculis, (R. A. = $16^h\ 37^m$, Decl. = + 31° 47′; 1895), as it seems to have a large motion in the line of sight. It is well known that this star is double; the principal component is of the third magnitude, and the companion, whose mean distance is about 1″

* Translated from *A. N.* 3184. Velocities in Herr Belopolsky's article are given in German geographical miles, usually to the nearest tenth. They are given here in kilometers to the nearest unit. As 1 geographical mile = 7.42 kilometres, the precision expressed is nearly the same.—*Tr.*

has a magnitude of 6.5. The time of perihelion passage is to be expected toward the end of the present century.

It is impossible to decide how far the spectrum of the principal star is influenced by that of the companion, but it can safely be assumed that with the slit-width which was used, and with an exposure of one hour, no trace of the spectrum of the companion would be produced on the plate. The deviations which occur in the measurements might otherwise, perhaps, be explained by this circumstance.

The spectrum was obtained with the larger spectrograph (two compound prisms) of the Pulkowa Observatory, in connection with the 30-inch refractor. Iron and hydrogen were used for the comparison spectra.

The sky was not in general very transparent during the observations, and on only one day, (May 18), was a photograph obtained on which the lines were sharp and traceable far into the violet, while on the remaining days, (May 22, June 2, 3, 4, 14 and 16) the photographs were taken through thin cirrus clouds, which strongly absorbed the violet part of the spectrum.

The slit was placed in the focus for the $H\gamma$ rays, and the camera objective was adjusted by experiments on the $H\gamma$ line.

It is an extremely difficult matter to keep a star on the slit with the great refractor, as the right ascension and declination screws are arranged only for rather coarse motions, while the pressure of the finger on the eye-end of the tube suffices to make the spectrum in the camera disappear. It was only after several mechanical devices had been added that we were able to successfully photograph the spectra of stars down to the fourth magnitude.

The spectrum of ζ Herculis belongs to Vogel's class II, and hence it was possible to obtain fairly accurate measures of the motion in the line of sight.

The measurements were carried out according to the Potsdam method, but the results are not final, since the value of a revolution of the micrometer has not yet been determined for different temperatures. The investigations have not yet been finished. The spectrograph was not attached to the 30-inch refractor until the beginning of April in the present year. On this account the computations have also not been made as rigorously as at Potsdam.

The following values of a revolution of the micrometer have been used:

At λ 430.8 $\mu\mu$	1 rev.	corresponds to	198	kilometres.
434.1	"	"	217	"
438.4	"	"	232	"
440.5	"	"	237	"
441.5	"	"	238	"

Each of the spectrograms (except that of May 18) was measured in two ways: (1), the displacement of the most suitable lines, referred to the Hγ line, was measured according to the method of Vogel; (2), the displacement of the star lines, with reference to the artificial iron lines was measured directly.

I must remark that the Geissler tube was made luminous, (at a distance of 35 centimetres in front of the slit), for only a short time (3 to 5 minutes) in the middle of the exposure, while the spectrum of iron was photographed either at the beginning or at the end.

The results of the measurements are now given, as follows: The displacement is represented by D.

1893, May 18, 11^h 15^m Pulkowa Mean Time.

First Method,	D = − 0.309	rev.	first series.
	D = − 0.307	"	second series, on another day.
Direct displacement	D = − 0.316	"	first series.
of Hγ line,	D = − 0.323	"	second series.

The resulting velocity is:

First method — 66.6 kilometrs.
Direct displacement of the Hγ line — 69 "

May 22, 11^h 0^m Pulkowa Mean Time.

First method D = − 0.356 rev. first series.
D = − 0.360 "
Direct displacement of the Hγ line D = − 0.393 "

Direct displacement of iron lines:

	λ 430.8	λ 432.6	λ 438.4	λ 440.5	λ 441.5
First series,	0.423 rev.	0.420 rev.	0.435 rev.	0.404 rev.	0.338 rev.
Second "	0.433 "	— "	0.433 "	0.394 "	0.340 "
Third "	— "	— "	0.393 "	0.384 "	0.337 "
Mean	0.428 rev.	0.420 rev.	0.420 rev.	0.394 rev.	0.338 rev.

The resulting velocities are:

From direct displacement of the Hγ line	— 87	kilometres.
" " " " " iron line λ 430.8,	— 85	"
" " " " " " " 432.6,	— 87	"
" " " " " " " 438.4,	— 97	"
" " " " " " " 440.5,	— 93	"
" " " " " " " 441.5,	— 81	"

By the first method, — 78 kilometres.

N. B.—The iron lines λ 438 and λ 440 in the star spectrum are not suitable for accurate measurements.

June 2, 11h 40m Pulkowa Mean Time.

First method, D = — 0.328 rev.

Direct displacement of the Hγ line		D = — 0.341 rev.
" " " " iron line	λ 430.8	D = — 0.302 "
" " " " " "	438.4	D = — 0.325 "
" " " " " "	440.5	D = — 0.304 "
" " " " " "	441.5	D = — 0.300 "

The resulting velocities are:

by displacement of the Hγ line		— 73 kilometers.
" " Iron line	λ 430.8,	— 60 "
" " " "	438.4,	— 75 "
" " " "	440.5,	— 72 "
" " " "	441.5,	— 71 "
	Mean,	— 70 "

First method, — 71 kilometres.

June 3, 11h 5m Pulkowa Mean Time.

First method, D = — 0.289 rev.

Displacement of iron line	λ 438.4,	D = — 0.290 "
" " " "	440.5,	D = — 0.288 "
" " " "	441.5,	D = — 0.279 "

The resulting velocities are:

by displacement of the iron line	λ 438.4,	— 67 kilometres.
" " " "	440.5,	— 68 "
" " " "	441.5,	— 66 "
	Mean,	— 67 "

First method, — 62 kilometres.

June 4, 11h 6m Pulkowa Mean Time.

First method, D = — 0.276 rev.
Displacement of the Hγ line, D = — 0.275 "

Displacement of the iron lines:

	λ 438.4	λ 440.5	λ 441.5
First series,	0.325 rev.	0.320 rev.	0.266 rev.
Second "	0.296 "	0.315 "	0.281 "
Third "	0.301 "	0.268 "	—
Fourth "	0.323 "	—	—
Mean	0.314 rev.	0.301 rev.	0.274 rev.

The resulting velocities are:

by displacement of the Hγ line		— 59 kilometers.
" " " iron "	λ 438.4,	— 73 "
" " " " "	440.5,	— 71 "
" " " " "	441.5,	— 65 "
	Mean	— 67 "

First method, — 60 kilometeres.

June 14, 11h 4m Pulkowa Mean Time.

First method	D = — 0.255 rev.
Displacement of iron line λ 432.6,	D = — 0.271 "
" " " 438.4,	D = — 0.254 "
" " " 440.5,	D = — 0.279 "
" " " 441.5,	D = — 0.248 "

The resulting velocities are:

by displacement of the iron line λ 432.6,	— 56	kilometres.
438.4,	— 59	"
440.5,	— 66	"
441.5,	— 59	"
Mean	— 60	"

First method, — 56 kilometres.

June 16, 11h 10m Pulkowa Mean Time.

First method,	D = — 0.263 rev.
Displacement of the Hγ line,	D = — 0.265 "

Displacement of the iron lines:

	λ 432.6	λ 438.4	λ 440.5	λ 441.5
First series,	0.311 rev.	0.310 rev.	0.285 rev.	0.301 rev.
Second series,	0.299 "	0.283 "	0.278 "	0.295 "

The resulting velocities are:

by displacement of the Hγ line,	— 57	kilometres.
" " " iron line λ 432.6,	— 63	"
438.4,	— 69	"
440.5,	— 67	"
441.5,	— 71	"
Mean,	— 65	"

First method, — 57 kilometres.

We obtain the following table of observed velocities of ζ Herculis:

First Method.

1893.	Observed motion.	Reduction to Sun.	Motion relative to Sun.
May 18	— 67 km.	+ 1 km.	— 66 km.
22	— 78	— 1	— 79
June 2	— 71	— 4	— 75
3	— 62	— 4	— 66
4	— 60	— 4	— 64
14	— 56	— 6	— 62
16	— 57	— 7	— 64
			Mean, — 67 km.

DISPLACEMENT OF THE $H\gamma$ AND IRON LINES.

1893.	Observed motion.	Reduction to Sun.	Motion relative to Sun.
May 18	— 69 km.	+ 1 km.	— 68 km.
22	— 88	— 1	— 89
June 2	— 70	— 4	— 74
3	— 68	— 4	— 72
4	— 68	— 4	— 72
14	— 60	— 6	— 66
16	— 65	— 7	— 72
			Mean, — 73 km.

The mean of the two methods for each day gives:

1893.	Motion relative to Sun.
May 18	— 68 km.
22	— 84
June 2	— 75
3	— 67
4	— 66
14	— 64
16	— 69
	Mean, — 70 km.

Although the deviation of each day from the mean are greater than might be expected from a consideration of the Potsdam probable error, we must not at once draw conclusions from this circumstance, for there seems to be a constant difference between the two methods used, which very probably depends upon the aspect of the iron lines in measuring. There is always a tendency to estimate the displacements greater than they really are. I did not succeed, however, by independent measurements and by studying the arrangement of the precipitated grains of silver, in reducing the differences of the direct displacements, particularly in the case of May 22, for which day the two methods give considerable deviations from the mean in the same direction.

For comparison I give here the motion of α Boötis, determined by the displacement of iron lines. It will be seen there is no material difference between my determinations and earlier ones, a fact which indicates the spectrograph was correctly adjusted.

α BOÖTIS. 1893, MAY 6, PULKOWA.

Iron line λ 429.9,	+ 2.4 kilometers.
432.6,	+ 4.2
438.4,	+ 7.4
440.5,	+ 3.3
441.5,	+ 4.7
Mean	+ 4.4 kilometres.

Reduction to Sun, — 10.1 km.
Motion relative to Sun, — 5.7 " = — 3.6 English miles.

The determinations of Vogel, Scheiner, and Keeler give;

v	— 4.4	English miles.	(6	determ.	First method).	
s	— 5.2	" "	(6	"	" ").	
k	— 4.3	" "	(3	"	Displacement of the D lines).	

ON THE NEW STAR IN AURIGA.*

H. C. VOGEL.

Seeliger† has advanced a hypothesis, founded on the most important observations of the Nova, which regards the occurrence from several new points of view, and is in many respects worthy of careful attention. He believes that his considerations enable him to avoid the difficulties which are encountered in explaining the great relative velocities, or on the other hand in the assumption of very great masses, on the hypothesis of the existence of two compact heavenly bodies.

In recent times it has become a generally accepted view, largely through the instrumentality of celestial photography, that the interstellar spaces are filled with more or less extensive aggregations of very sparsely distributed matter. There is consequently no improbability in the entrance of a heavenly body into such a cloud, and certainly a much greater probability than that two compact bodies should pass at a very short distance. "But as a body enters such a cosmical cloud, its surface will begin to be heated no matter what the constitution of the sparsely distributed material may be. In consequence of the superficial heating, vaporized products will form around the body, which will in part become detached from it and quickly assume the velocity of the neighboring parts of the cloud."

If observed spectroscopically, the star thus rendered incandescent would show two superposed spectra; one continuous, and in consequence of the layer of glowing vapor in front of the body crossed by dark absorption lines, the other from the nature of the occurrence, consisting of bright lines. Both spectra would be displaced in proportion to the relative motion in the line of sight. On the whole the resulting appearance would very closely resemble that which was actually presented by the Nova.

"Of very great importance, however, is the investigation of the question whether we can in this way arrive at a plausible explanation of the great relative velocity indicated by the two spectra.

* Continued from January number, page 57.

† A. N. 3118. ASTRONOMY AND ASTRO-PHYSICS, Dec. 1892.

On the approach of the body, the cloud would evidently be lengthened in the direction of approach. This lengthening, and likewise the relative velocity of the individual cloud-particles with respect to the body, would grow with the increasing proximity of the latter. Without some definite provision in regard to the structure of the cloud, it is difficult to give any detailed representation of the phenomenon that will ensue, and we must content ourselves with considering some special case which will allow of closer investigation. If we assume, for example, that the separate particles of the cloud are in general influenced only by the attraction of the body, they will describe hyperbolas around the latter with its center as focus. Their greatest relative velocity will diminish rapidly with their distance from the body so that the neighborhood of the latter will be filled with particles having very different velocities. It is easy to see that no extravagant assumption is required to obtain very great velocities for the particles which pass close to the surface of the body,—velocities such as have been proved to exist in the case of Nova Aurigæ, and this even when the initial velocity of the particles is very small. It also follows from what has been shown above, that the spectral lines of particles moving away from the body with such different velocities must be greatly widened; moreover, not only is not the slightest difficulty encountered in explaining the different brightness of various parts of these lines, but the existence of such maxima of brightness follows as a necessary consequence. This point does not seem to me to be unimportant since it cannot be deduced from the hypothesis of the close passage of two compact masses, but leads to the very improbable assumption that there are several moving bodies of this kind.

"As long as the body moves within the cosmical cloud, the appearances just described will be continually reproduced, and it follows that the characteristic features of the spectrum, apart from minor changes determined by all the circumstances of the case, must as a whole remain unchanged for a considerable time. . . . It is also not surprising that during this time the brightness of the star should undergo little variation, but that it should fall off pretty rapidly after the emergence of the body from the cloud. This also agrees well with the observed light curve of the Nova."

Seeliger assumes that the star entered the cosmical cloud in the beginning of December, and left it not long before the beginning of March. He seeks to decide the question, how the great relative velocity could persist for so long a time, by comparing the motion of the star against the resistance of the cloud with that

of a meteor in the upper regions of our atmosphere, and arrives at the conclusion that the retardation would not necessarily be perceptible.

That in spite of this small retardation, enough kinetic energy is transformed into heat to cause the superficial incandescence of the star, is made the subject of a mathematical investigation by Seeliger, with the result that "we can assume that the density of the cosmical medium is very small compared with that of the extremely tenuous air in which the meteor is brought to incandescence, and still obtain the necessary quantity of heat. It is worthy of remark that all the numerical values can be varied within very wide limits without danger of contradiction."

In the second apparition of the Nova, Seeliger finds a confirmation of his views, for it is inherently probable that the supposed nebulous or dust-like clouds are more numerous in certain regions of space than in others, and it is also permissible to make various assumptions as to the distribution of density in the matter comprising them.

At the first glance there is something remarkably captivating about this hypothesis, but on a closer comparison with the observations, to which I will confine myself, doubts of no inconsiderable moment arise whether it really appears adequate to a complete explanation of Nova Aurigæ. However the case may be, I fully concur with the opinion of Seeliger that the hypothesis, which deals with entirely possible conditions, is to be regarded as a valid explanation of the appearance of certain new stars.

Under the supposition that the body moving through the cloud, and the cloud itself, have no unusually great velocities in comparison with the mean velocity of stars in space (for the main object of the hypothesis is to avoid the assumption of such velocities) and that the body entering the cloud is moving toward us, the particles of the cloud, shortly before the entrance of the body, would, by virtue of its attraction, move toward and past it. They would carry with them particles of its atmosphere, and, by their passage close to its surface, acquire a more or less great velocity in a direction opposite to the Earth. The spectrum of the body, whose surface on entering the cloud is thus raised to incandescence, would probably be continuous and show absorption lines, and on it would be superposed the bright line spectrum of the detached particles of the body's atmosphere. The centers of these lines would at first be relatively displaced, the bright lines toward the red, since with Seeliger we have to assume that the

particles of the cloud would, in general, move away from us. But when the body is once in the cloud, particles rush upon it from all sides, those grazing its surface would acquire motions making all possible angles with the line of sight. The bright lines would consequently appear very greatly broadened, but a displacement of the centers of the bright and dark lines could no longer be supposed to exist, since the motions of body and cloud would not be appreciable in comparison with the enormous velocities which would be imparted to the separate parts of the cloud by the attraction of the body. At the exit of the body we should expect appearances similar to those at its entrance, but the bright lines would be displaced toward the blue.

Now the body, at the time of the first spectroscopic observations, was already near the middle of the cloud, and its exit, according to Seeliger, occurred in the beginning of March. During this interval, however, the spectrum remained essentially unchanged, and the observatians have therefore not corresponded with the appearances which were to be expected.

It should further not remain unmentioned that Seeliger has fixed his attention on only the relative motion, determined by the displacement of the bright with reference to the dark lines, and has overlooked the great velocity of over 400 miles per second with which the star having absorption lines in its spectrum is actually moving in space. But this fact must not be left out of consideration in any hypothesis regarding the nature of Nova Aurigæ.

If, now, a body should move toward us with such a velocity, through a cosmical cloud, the conditions would be materially different and the resulting spectrum would perhaps approximate more closely to the observed one, inasmuch as it may be assumed that the bright lines would be constantly displaced toward the red, even when the body was in the middle of the cloud. At the same time the bright and dark lines would overlap as far as their centres and therefore would not appear to be separated by their full breadth; for a great number of the particles of the cosmical cloud which produce the bright lines would have the same velocity as the penetrating body, as with such great velocities vortex motions would be inevitable. In this connection there remains unexplained, why the smallest motion relative to the body with the absorption spectrum is not zero, but something like 360 miles per second, how under the assumed conditions any other appearances than a broadening of the bright lines could be produced, how therefore, with uniform distribution of matter, or of the sep-

arate particles in the cosmical cloud, so prominent maxima of intensity could be formed in these bright lines, and why these maxima should have been subject to only very slight changes in their relative positions.

In the second apparition of the Nova, when practically only an emission spectrum was produced, the essential data for testing the hypothesis are wanting; but we should naturally expect to find appearances like those which accompanied the first outbreak, and above all, if we assume with Seeliger that the compact body entered an outlying portion of the supposed nebula, a strong heating of its surface.

The opinion that the Nova could be explained by the encounter of a heavenly body with several bodies, forced itself upon me immediately after the first observations, and I have since been more and more confirmed in this view by the further observations which have been made. It is true that the question, whether there was not too small a probability in such an encounter of heavenly bodies, at first gave rise to doubts, but they soon disappeared on considering that, according to the Kant-Laplace hypothesis, it is difficult to conceive of a large celestial body unaccompanied by attendants. It is really surprising that in all the hypotheses accounting for new stars, the assumption of these simple conditions should have been left out of consideration.

If we assume that a body, having a mass of the same order as that of the sun, should suddenly pass close to a system like our own, whose central star had lost its light by gradual cooling, then enormous disturbances would be produced, and the collision of individual members of the system and the consequent production of luminous outbursts, would be inevitable.

Let us now suppose that the body which gave the continuous spectrum with absorption bands in the composite spectrum of the Nova, and which, as is well known, was moving in space with a velocity of about 400 miles per second, came close to a system moving with no more than the ordinary velocity, in a direction with regard to which no special assumptions need be made.

By the passage close to one of the larger or several of the smaller bodies of the system, perhaps also by direct collision with smaller bodies, the star entering the system would suddenly be brought to a high state of incandescence. At the time of the spectroscopic observations the body was in a part of the supposed system which was somewhat thickly filled with little bodies, and these, by their close passage and by partial collisions,

maintained the brilliant incandescence of the surface and atmosphere of the penetrating body, which the extension of the continuous spectrum into the violet shows that they must have possessed. In this way some of the bodies themselves acquired an excessively high temperature and a more or less high velocity, giving rise to the spectrum with bright lines, and thus producing the same effect as the particles of the cosmical cloud in Seeliger's hypothesis. There is however this importane difference, that the motion of the little bodies was regulated by the central star; they moved in an actual stream against the penetrating body, and therefore could not have moved toward it from all directions.

It is now explicable that the bright lines should have appeared broadened, displaced, and diffuse; nor does it any longer appear strange that the least displacement of the bright lines (one of the edges) did not coincide in position with the middle of the dark lines, but represented a small motion in space which was perhaps not greatly different from that of the supposed stellar system.

The atmospheres of the central body and larger bodies of the system would also be greatly heated by inevitable disturbances of the surface-levels and consequent eruptions, and if these causes were not sufficient to produce a higher temperature in the surfaces of the bodies than in their atmospheres, (which however we might expect in the case of a body heated by the fall of smaller bodies from without), they would nevertheless give rise to a spectrum in which bright lines would predominate. In this then we have a simple explanation of the intensity-maxima in the bright hydrogen lines, which indicate a small motion in space, and which at first had the greatest intensity.

We also find a satisfactory explanation, based on assumptions whose probability has ample support in a system subject to so many disturbances, of other observed phenomena, such as the second maximum of brightness which were seen so long in the hydrogen lines, and the temporarily appearing third maximum in the same lines, and even, assuming that they are not to be regarded as reversals, of the fine bright lines which appeared in them. It should be remarked, by the way, that from this point of view they are not explained by any of the hypotheses previously mentioned.

In continuation I may still specially add that the anomalies observed in the measurements of the D lines,—namely, that the displacement of these lines in the star spectrum referred to the artificial lines was found to be less than that of the hydrogen lines, (Huggins, Becker),—as well as similar observations of the

finer chromosphere lines (Campbell) are at once intelligible, for the spectra of the different bodies need not neccssarily exhibit the same lines. And I may still further mention that the second outburst of the Nova in the autumn of 1892 may be ascribed to the meeting of the flying body with some distant member of the stellar system through which it was supposed to be passing. The most reliable proof of the correctness of the views here set forth would, in fact, be secured, if it could be shown with greater certainty that there have been such changes in the wave-lengths of the bright lines in the spectrum now visible as are indicated by the observations of Campbell, for the assumption of an orbital motion would then be allowable.

I will not however lose myself further in details, for my purpose was in the main only to show that the meeting of a body moving at random through space and an ordered system of bodies has not too small a probability, for no objection can be raised to the assumption of a planetary system in connection with a fixed star and also to show that in the assumption of such a system traversed by a body moving with the abnormal volocity of from 400 to 460 miles per second and in which the body might remain for weeks or months, as it would require, for example, five months to pass through our own system), the principal phenomena observed in Nova Aurigæ find a natural, unstrained explanation.

ON THE NEW STAR IN AURIGA.*

H. SEELIGER.

The phenomena presented by this remarkable star have not yet come to an end. It is for this reason that I have hitherto refrained from adding anything further to my first communication on the subject in A. N. 3118.† The opinions which I there announced were necessarily founded on assumptions of the greatest possible generality, since it was not permissible in the discussion to anticipate the results of observations which at that time were still incomplete. The reappearance of the Nova in August, 1892, occurred, it may be remarked, nearly at the time when my paper was published. It seems to me that the time for taking up the investigation again has not even yet come, but that it should

* Translated from A. N. 3187.

† See also ASTRONOMY AND ASTRO-PHYSICS, December, 1892.

wait for further developments. Herein also are contained the reasons why I have not entered into a discussion of remarks which have been made here and there on the nature of the occurrence, and it is not my intention to discuss them now. Nevertheless, certain recently published remarks which Herr H. C. Vogel* has appended to a collection of observed appearances of the Nova, seem to make it advisible that I should enter into a brief consideration of the subject, for Herr Vogel's paper contains views which I cannot recognize as correct, and the propagation of which I cannot regard as useful to science.

I had assumed that the outburst of the new star was caused by its contact with a cloud of thinly scattered matter. In regard to the physical constitution of the material, purposely nothing was said, in order not to anticipate special appearances which might possibly be required to satisfy the observations. When therefore my views are characterized from the one side as a modification of the meteoric hypothesis, the agreement with the real facts is as small as in the interpretation that the cosmical cloud could not be dust-like in nature. A clear view of the motion of such a cloud relatively to the star is obtained by applying the elementary principles of mechanics. "On the approach of the body the cloud would evidently be lengthened in the direction of approach. This lengthening, and likewise the relative velocity of the individual cloud-particles, would grow with the increasing proximity of the latter."†

The motion of the separate particles will evidently depend upon the physical structure which is assigned to the cloud. It will be different with a dust-like constitution from what it would be with a fluid or gaseous one; but in any case the phenomena produced will be essentially those demanded by the hypothesis. Thus, what will take place is as follows: (1) In consequence of the approach of the cloud and the body due to attraction, a stream of matter appearing to come from a definite direction, will pour upon the body, no matter whether the relative velocity of the two is large or small when they are very far apart. (2.) Certain conditions of motion will be developed in the more or less continuous stream as soon as the body enters the cloud, and in the main will remain unchanged until it passes out, or at least as long as the separate parts of the cloud preserve the same constitution. If the matter is assumed to be dust-like in character, the

* Ueber den neuen Stern im Fuhrmann, Abhandlung der Berliner Akademie vom Jahre 1892. Translation in ASTRONOMY AND ASTRO-PHYSICS, Dec., 1892, and Jan., 1893.

‡ From Professor Seeliger's first paper.—*Tr.*

attraction of the body on the separate particles will be the principal agent. The separate particles will then describe hyperbolas whose asymptotes are all parallel and whose focus is in the body. The particles will approach the body from the direction of the asymptotes. Since a simple application of Kepler's laws will determine all the circumstances of motion in this case, it seems superfluous to consider them more closely here; only the following remarks may still be added. The hyperbolas described in the relative motion will have a considerable curvature close to the body if the initial velocity is appreciably smaller than the maximum velocity. Then after passing the body the particles will move along the hyperbolas, in directions which, under certain circumstances, may form acute angles with the original directions. The magnitude of these angles will depend upon the initial velocity, (with given maximum velocity); within certain imits, therefore, it might be assumed. But in the case of a continuous stream the circumstances would not assume this simple form, for the reason that particles describing congruent, but differently lying hyperbolas, would, on account of the prevailing symmetry, come in contact behind the body. If the observer should be situated not very far from the direction of the asymptotes,—and it is easy to see that very wide limits are here allowable,—these collisions would not be directly noticeable. If the medium has a fluid or gaseous structure, this point either does not come into consideration at all, or it takes an entirely different form. The circumstances would then in general resemble those of flying projectiles, which have become widely known by photographic researches, and particularly by the beautiful experiments of E. Mach.

The conditions last mentioned require, under some circumstances, a closer investigation. Strange to say, however, Herr Vogel has denied the correctness of the deductions first arrived at, although they are incontestable, and has arrived at the following conclusions, which are irreconcilable with the teachings of mechanics. He says (pp. 56 and 57):

"Under the supposition that the body moving through the cloud, and the cloud itself, have no unusually great velocities in comparison with the mean velocity of stars in space (for the main object of the hypothesis is to avoid the assumption of such velocities) and that the body entering the cloud is moving toward us, the particles of the cloud, shortly before the entrance of the body, would, by virtue of its attraction, move toward and past it. They would carry with them particles of its atmosphere,

and, by their passage close to its surface, acquire a more or less great velocity in a direction opposite to the Earth. The spectrum of the body, whose surface on entering the cloud is thus raised to incandescence, would probably be continuous and show absorption lines, and on it would be superposed the bright line spectrum of the detached particles of the body's atmosphere. The centers of these lines would at first be relatively displaced, the bright lines toward the red, since with Seeliger we have to assume that the particles of the cloud would, in general, move away from us. But when the body is once in the cloud, particles rush upon it from all sides, and those grazing its surface would acquire motions making all possible angles with the line of sight. The bright lines would consequently appear very greatly broadened, but a displacement of the centres of the bright and dark lines could no longer be supposed to exist, since the motions of body and cloud would not be appreciable in comparison with the enormous velocities which would be imparted to the separate parts of the cloud by the attraction of the body. At the exit of the body we should expect appearances similar to those at its entrance, but the bright lines would be displaced toward the blue."

All that Herr Vogel says here about the directions of motion really applies only to the accelerations. *The two conceptions acceleration and velocity are thus confounded,* and consequently the principal objection of Herr Vogel to my assumptions must be regarded as entirely removed.

As for the further remarks which relate to my hypothesis, and which Herr Vogel probably regards as of minor importance, I shall at present touch upon only one more point, reserving the rest for a future occasion. Herr Vogel says, "It should further not remain unmentioned that Seeliger has fixed his attention only on the relative motion which is indicated by the displacement of the bright as compared with the dark lines, and has overlooked the great velocity of over 400 miles per second with which the star whose spectrum contained absorption lines was actually moving through space. But this circumstance must not be left out of consideration in any hypothesis on the nature of Nova Aurigæ." After these words, everyone would expect that Herr Vogel, in the hypothesis of his own which is given further on, would take into account this point, which he evidently considers to be an important one. This is not done, however; indeed, no attempt is made to explain the great velocity, nor, I may be permitted to mention, can it be made successfully in the case he supposes. By establishing this relation of the two cases, I am freed from every necessity of going further into the subject.

With regard to the hypothesis advanced by Herr Vogel as an explanation of the new star, I shall content myself with quoting the paragraphs which contain its principal features, and adding to them a few brief comments. This will be sufficient, I think, to allow a judgment to be formed as to the validity or probability of these and similar views.

(1). "The oipnion that the Nova could be explained by the encounter of a heavenly body with several bodies, forced itself upon me immediately after the first observations, and I have since been more and more confirmed in this view by the further observations which have been made. It is true that the question, whether there was not too small a probability in such an encounter of heavenly bodies, at first gave rise to doubts, but they soon disappeared on considering that, according to the Kant-Laplace hypothesis, it is difficult to conceive of a large celestial body unaccompanied by attendants. It is really surprising that in all the hypotheses accounting for new stars, the assumption of these simple conditions should have been left out of consideration."

If it is thought desirable to bring in the hypothesis of Laplace, the facts which it was principally intended to explain must not be left unconsidered. The most important perculiarity of a planetary system which here comes in question consists in the grouping of masses around a certain middle plane, and a (say) spherical grouping of masses is not compatible with the conception of a planetary system which is familiar to us, and which, in default of other experience, must be regarded as the only one admissable. Assuming now that every thing else in Herr Vogel's hypothesis is correct, it is possible to satisfy the observed appearances only on the assumption that the star was many months in close proximity to the individual members of the system, particularly when it is remembered that the reappearance of the Nova is to be explained in the same manner. From this it follows that the star must have moved in a direction which was inclined at an extremely small angle to the mean plane of the system. It will hardly be admitted that such a condition has any special probability, and the doubts which may arise in this aspect of the question are by no means dispersed.

2. "If we assume that a body, having a mass of the same order as that of the Sun, should suddenly pass close to a system like our own, whose central star had lost its light by gradual cooling, then enormous disturbances would be produced, and the collision of individual members of the system and the consequent production of luminous outbursts, would be inevitable."

The possibility of such a collision can of course not be denied; but it should be observed that the orbits will be changed in consequence of the great relative initial velocity, which is to be assumed according to (3), although on a materially smaller scale. The assumption of a moderate number of large planets will even then not make it easy, least of all for these, to show that a collision is "inevitable." Further, according to the observed phenomena, the collisions must have occurred very soon after the appearance of the Nova and probably at exactly that time, and no more occurred afterward, at least not after the beginning of spectroscopic observations, for a considerable change in the spectrum was not observed during the first appearance. All this cannot be said to show that the views advanced are very probable, quite apart from the consideration that we are justified in requiring a less general statement of the circumstances, in order to allow discussion in one or another direction.

(3) "Let us now suppose that the body which gave the continuous spectrum with absorption bands in the composite spectrum of the Nova, and which, as is well known, was moving in space with a velocity of about 400 miles per second, came close to a system moving with no more than ordinary velocity, in a direction with regard to which no special assumptions need be made."

This sentence shows the correctness of the remark I have made above, that Herr Vogel makes no attempt whatever to refer the velocity of 400 miles per second to normal conditions. Such an attempt can in no way lead to this end on the ground of the remaining assumptions, without encountering contradiction of the observations or inadmissibly large masses; this follows immediately from the formulæ of my article.

(4). "By the passage close to one of the larger or several of the smaller bodies of the system, perhaps also by direct collision with smaller bodies, the star entering the system would suddenly be brought to a high state of incandescence. At the time of the spectroscopic observations the body was in a part of the supposed system which was somewhat thickly filled with little bodies, and these, by their close passage and by partial collisions, maintained the brilliant incandescence of the surface and atmosphere of the penetrating body, which the extension of the continuous spectrum into the violet shows that they must have possessed. In this way some of the bodies themselves acquired an excessively high temperature and a more or less high velocity, giving rise to the spectrum with bright lines, and thus producing the same effect as the particles of the cosmical cloud in Seeliger's

hypothesis. There is however this important difference, that the motion of the little bodies was regulated by the central star; they moved in an actual stream against the penetrating body, and therefore could not have moved toward it from all directions."

Leaving out of consideration the last statements, which, as shown above, are founded on error, I must object to this as in my first paper, that without greater definiteness the simple passage of two dark bodies can be assumed as the cause of their luminous outburst, and continuous or discontinuous spectra can be assigned to them at pleasure. Nothing whatever is known about the effect of such influences, and they would in any case depend upon so many attendant circumstances that it seems to me rash to support a hypothesis on such a foundation. To assume small planetary bodies as the cause of such occurrences, in particular, seems to me entirely inadmissible. The further development of the hypothesis can be recognized as correct only under entirely definite assumptions. If the small bodies should not meet the large one in a swarm which could be regarded as nearly continuous, other phenomena would manifest themselves than Herr Vogel seems to think. The hyperbolas described by the small particles would have a great curvature in the close vicinity of the body, since the initial velocity is only a fraction of the maximum velocity. Continuing on these hyperbolas, the luminous particles would soon be moving very nearly in the direction of the second asymptote. If we assume, as a condition which is favorable to the hypothesis, that the Earth is very nearly in the original direction of the particles' motion, *i. e.*, in the asymptote of the parabola, the particles would very shortly after their closest approach to the body, be moving in a direction inclined at an angle 2 α to the line of sight, α being the angle between the major axis and asmyptote of the hyperbola. From the values assumed by Herr Vogel (initial velocity 400 miles, greatest velocity > 740 miles) it would follow that luminous particles must be in evidence spectroscopically, having a motion of at least 230 miles per second *toward* the observer. It will be seen therefore that in this case phenomena would be produced which are not confirmed by the observations, and hence a modification of the fundamental assumptions becomes necessary. Such a modification, as already mentioned, consists in attributing to the stream of small bodies the character of a continuous stream. *But that is the essential feature of my hypothesis*, and the assumption of Herr Vogel would differ from it only in the highly problematical

way in which, according to him, the small bodies are brought to a state of incandescence. That the particles were moving under the influence of the central mass before they came within the sphere of attraction of the penetrating star, does not at all change the nature of the occurrence, and introduces only unimportant modifications. It may be well to mention that in the further development of this conception, which is in itself a very simple one, the known theorems of Laplace relating to very large perturbations readily lead to a correct judgment of the circumstances. These theorems can be applied with considerable certainty because the relative velocities involved are very great, (at least 400 miles per second), and the errors of the approximation are thereby materially reduced.

(5) "The atmospheres of the central body and larger bodies of the system would also be greatly heated by inevitable disturbances of the surface-levels and consequent eruptions, and if these causeswere not sufficient to produce a higher temperature in the surface of the bodies than in their atmospheres, (which however we might expect in the case of a body heated by the fall of smaller bodies from without), they would nevertheless give rise to a spectrum in which bright lines would predominate. In this then we have a simple explanation of the intensity-maxima in the bright hydrogen lines, which indicate a small motion in space, and which at first had the greatest intensity."

The "disturbances of the surface-levels" of the central body are no doubt assumed to be caused by the entrance of the body from without. If that were not the case, still another hypothesis would be called for, for it cannot be doubted that in any case such eruptions can be produced only when the attractions are very great and act at extremely short distances. But if the passage of the cosmical body close to the central mass plays so important a role, it fits in very badly with the probability of the entire occurrence; for we must then assume the accidental coincidence of two events; (1) the cosmical body must move very nearly in the plane of the planetary system, (2) it must also pass extremely close to the central body. Moreover, Herr Vogel explains the reappearance of the Nova by an encounter with a distant planet, which adds a new assumption of little probability to the previous one.

I shall refrain from going further into particulars. What I have said will probably suffice to show clearly that hypotheses like that of Herr Vogel are in no way capable of serving as the basis of more extended considerations.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopyand other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Professor Vogel on Nova Aurigæ.—In commenting on Professor Vogel's hypothesis last month, we referred to the translation of his memoir as completed in that number, but owing to the exigencies of printing, the latter part, containing Professor Vogel's own views, had to be omitted. It is given in the present number, together with an article by Professor Seeliger in which the correctness of his original hypothesis is upheld.

In *A. N. 3198*, Professor Vogel replies to the remarks of Herr Belopolsky quoted in our Astro-Physical Notes last month. The fine lines in the spectrum of Venus, and the identity of details in the accidentally triple spectrum of the Nova obtained with the Pulkowa refractor, are not regarded by Vogel as a convincing proof that the details are real. The Harvard College photographs are materially different from Belopolsky's. With regard to the occurrence of iron lines in the part of the spectrum covered by the Pulkowa photographs, Professor Vogel says that six or seven such lines are found in his own photographs and ten in those of Campbell. It is natural that the supposed gaseous envelope should be more distinct with the 15-inch than with the 30-inch refractor, as the circles of chromatic aberration are brighter with the former instrument, and therefore easier to see.

Professor Vogel takes the occasion to say that his views have not been changed by the criticisms of Professor Seeliger, but that he does not at present intend to pursue the subject farther.

The Sky from Pike's Peak.—In the October number of ASTRONOMY AND ASTRO-PHYSICS, Vol. XII, p. 750, Dr. Barnard describes the wonderful blueness of the sky as seen by him from the summit of Little Ouray mountain, Colorado, on Aug. 16 last. He also speaks of the fact that his view from the summit of Pike's Peak on the preceding day was spoiled by a sudden snow-storm.

I ascended Pike's Peak ten days later than the date of Dr. Barnard's visit and remained there during the night of Aug. 24 and until noon of the 25th. During the first half of the night the sky was obscured by fast-drifting clouds, but leaving my cold bed in the half-ruinous stone building formerly used as the Government Signal Station, and going out of doors about two o'clock in the morning, I was delighted to find the clouds all gone and the sky ablaze with stars down to the very rim of the horizon. To eyes accustomed to the dim and smoky horizons of the East it was positively startling to see stars of the third and fourth magnitude shining with apparently undiminished lustre close to the edge of the celestial canopy. The number of visible stars seemed to have been multiplied indefinitly. I longed for a telescope. The most powerful instrument I had was an opera glass, and its revelations were superb.

After sunrise the sky was equally clear, only a layer of haze being visible far below upon the plains, and I noticed, at once, the absence of any perceptible atmospheric glare around the Sun. Upon covering the disc of the Sun with a bit of black glass I was able to follow the pure dark blue of thesky right up to the Sun's

edge. As far as I could determine there was absolutely no haze in the air above the peak. This experiment with the glass was made when the Sun was near the meridian, but earlier in the day, and when the Sun had risen but a few degrees above the horizon the absence of atmospheric glare was equally noticeable to the unaided eye.

I was the more interested by these observations because of the then recent visit of Professor Hale who had found the atmosphere there unsuitable for his attempt to photograph the corona without an eclipse. I am convinced that if Professor Hale's visit had been made near the end of August he would have found the air far better suited to his experiment than it was at the time when that experiment was tried. GARRETT P. SERVISS.

The Spectroscopic Investigations of Mr. George Higgs.—The publication of a new photographic map of the normal solar spectrum, extending from λ 8345 in the infra-red to the extreme limit of the ultra-violet, is an event calling for more than passing comment. For such a map, to prove itself truly worthy of publication, must rival in excellence the autographic records of the solar spectrum already in the hands of spectroscopists. In these days of refinement in instruments and methods it is a matter of no small difficulty to enter a field already occupied by leaders of research, and produce results of such evident merit as to leave little or no room for criticism. But this is what Mr. George Higgs, of Liverpool has done. His map of the solar spectrum, in its sharpness and clearness of definition, its greater extent, and the perfection of its photographic reproduction, offers at least some points of superiority over even the universally accepted standard of excellence which we owe to Rowland.

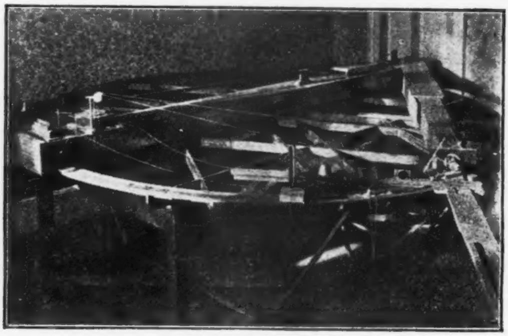
It therefore goes without saying that during a recent stay of a few days in Liverpool, we were quite ready to make use of the opportunity thus afforded us of visiting Mr. Higgs in his laboratory. In the present note it is not our intention to describe all that Mr. Higgs' kindness enabled us to see, but merely to point out one or two features of the work.

The Rowland concave grating employed is four inches in diameter, and has a radius of curvature of 10 feet, 2 inches. The scale of Higgs' original negatives is thus about half that of Rowland's. The form of mounting differs materially from that usually adopted. The grating and photographic plate are fixed at the opposite extremities of a bar forming the diameter of a circle 10 feet 2 inches in diameter. The slit is placed at the extremity of a movable radius, and can thus be set at any point on the circumference. In practice, its range is limited to the orders of spectra under investigation. The circular table which carries the grating, plate and slit is built of strips of varnished pine, and is thoroughly braced. As it stands on the second floor of a building facing on a busy street, precautions had to be taken to obviate the constant vibration. When the instrument is in use it rests upon three legs. Of these, two terminate in rollers, the spindles of which are carried on packing of cork and india-rubber. The third rests on a spring-board. In this way little difficulty is experienced from vibration.

The accompanying cut, which has been made from a small photograph taken by Mr. Higgs, will serve to give an idea of the arrangement of the instrument. It will be seen that the grating-box is connected with the slit and camera by long wooden boxes, which effectually exclude the light, and render darkening of the room unnecessary. A heliostat, placed on a shelf outside the window, reflects sunlight upon a 2-inch quartz lens of about 3 feet focus, which forms an image of the Sun on the slit. When photographing the extreme ultra-violet a silvered

quartz screen is used to cut out the less refrangible rays. The maximum transparency of the screen is for light of λ 3200.

Mr. Higgs has acquired great skill in the use of dyes for sensitizing photographic plates for the lower region of the spectrum, and a special preparation of alizarin blue, described in a recent volume of the *Royal Society Proceedings*, enables him to secure excellent negatives as far down as λ 8340. As a long strip of spectrum is photographed on a single plate, different exposures are of course required for different regions. A screen, pivoted at one side of the camera, and moved by clock-work about a vertical axis during the exposure, makes it possible to produce negatives of equal density throughout the spectrum.



MR. GEORGE HIGGS' CONCAVE GRATING SPECTROSCOPE.

But the most striking difference between the methods of Mr. Higgs and those of other investigators is in regard to the adjustment of the apparatus. Ordinarily, a concave grating is adjusted once for all, and small changes are made from time to time as occasion seems to require. It is well known that one of the most rigorous requirements in the use of the instrument is the exact parallelism of the slit and the lines of the grating. It is usual, however, to regard this adjustment, when once made, as permanent, or so nearly so as to require infrequent attention. Mr. Higgs does not rely on any possible permanence, but completely readjusts the apppratus for every new exposure. A long lever, controlled by cords within easy reach of the observer at the eye-piece, gives the means of rotating the slit while observing the spectrum. The maximum sharpness of the lines can thus be obtained in a moment. The grating can also be rotated about a vertical axis through a small angle, and the remaining adjustments effected by the observer without leaving his seat. We have little doubt that Mr. Higgs' marked success is due to the care taken with the adjustments of the instrument, which, it should be added, was made wholly (with the single exception of the grating) by himself.

We are indebted to Mr. Higgs for a portfolio filled with a great variety of spectrum photographs. The solar spectrum is shown in various regions, in all of which the sharpness of the lines is remarkable. The series includes a large number of photographs made with low Sun, and the telluric spectrum has never been better portrayed. The A and B groups are particularly striking. Other photographs of the spectrum of opposite limbs of the Sun show the displacement of the lines due to the solar rotation. In many of the spectra the H and K lines

contain in the center of the broad absorption bands distinct double reversals. These are due to the solar faculæ. The astigmatism of the concave grating would cause the double reversal in any faculæ which happened to fall upon the slit to extend across the entire spectrum. We have seen similar double reversals in Professor Rowland's original negatives.

Mr. Higgs' photographic map of the normal solar spectrum is published in three sizes: the first in 44 parts and enlarged 4 times, the second in 45 parts, enlargement twice the original size, and the third in 15 parts, original size. The prices are £8 8s, £3 3s and £1 1s respectively. Mr. Higgs' address is 467 West Derby Road, Liverpool.

G. E. H.

The Change of Sensitiveness in Dry Plates.—In the November number of ASTRONOMY AND ASTRO-PHYSICS an interesting fact is stated by Dr. Max Wolf regarding the change in sensitiveness of dry plates when used for stellar photography. Dr. Max Wolf found that Lumière plates increased three-fold in sensitiveness after keeping for five months. The same thing has been stated by others regarding change in sensitiveness of dry plates when determined with the Warnerke Sensitometer.

During the past ten years I have made hundreds of experiments on the sensitiveness of plates, for day-light, candle light, and through red glass, but I have never noticed any change in sensitiveness when used for a landscape negative. Various brands of plates have been kept for a number of years without my suspecting any change in the sensitiveness when used in daylight. There is, however, a peculiar effect produced in the sensitiveness of any plate when used in feeble light, if it receives a preliminary or supplementary exposure.

In a paper read before the A. A. A. S., in 1892, and also in a paper presented before the Photographic Congress, in Chicago, last August, I showed that a collodion or emulsion plate may become from five to eight times more sensitive when used in candle light or feeble day-light, provided it had received a proper amount of preliminary exposure. A plate, however, subjected to such preliminary exposure, does not show increased sensitiveness when used in strong day-light, or for a landscape negative.

My explanation of this phenomenon is as follows: A photographic plate may receive a certaina mount of light, and when placed in the developer, show no visible blackening of the film. A plate, therefore, which has had a certain amount of preliminary exposure may be under such strain that a very small amount of additional exposure will enable the developer to reduce the bromide of silver. Under this conception the actinic effect should be measured by the intensity of the light multiplied by the time, plus a constant quantity; the constant being a function of the original sensitiveness.

It would appear from the observations of Dr. Max Wolf, that a chemical effect similar to preliminary exposure may take place in the plate when kept in the dark a certain length of time. The subject is one of great interest, and should be farther investigated.

G. W. HOUGH.

Northwestern University.

The Radiation of Heated Gases.*—The question as to the nature of the radiation given out by heated gases is clearly one of fundamental importance, bearing

* F. Paschen: *Ueber die Emission erhitzter Gase, Wied. Ann.*, Bd. 50; pp. 409-443.

as it does directly upon the spectroscopic study of comets and nebulæ. The subject has claimed the attention of numerous experimenters, who have endeavored to determine whether a gas or vapor can give a "characteristic," discontinuous spectrum in virtue of its temperature alone, or whether a chemical process is necessary to excite the characteristic molecular vibrations. Of these two views, the latter appears to have been the more favorably received. Until recently it appears that in all experiments in which a discontinuous spectrum has been obtained from a gas, some chemical action was at the time taking place at the luminous source. The question has been recently put to the test by Pringsheim, (see *Wied. Ann.*, Bd. 45, p. 428; Bd. 49, p. 347) who heated sodium and sodium salts in a porcelain tube, and examined the resulting emission spectroscopically. In no case was he able to render the gases luminous, except upon introduction into the tube of some reducing agent, and hence concludes that there is no ground for the assumption that gases can become luminous by a mere increase of temperature.

The recently published work of Paschen seems however to furnish abundant evidence in favor of the opposite view. The problem which he assigned himself was in the first place to devise a means of heating the gas under examination to a much higher temperature than had been previously attained, at the same time reducing to a minimum the mass of hot solid matter in the neighborhood of the slit of the spectroscope; and to examine the radiation by a more delicate method than has been hitherto employed. In both of these the writer was highly successful. As a means of heating the gas a narrow strip of platinum foil was wrapped around a glass rod in such a way as to form a cylindrical tube 4 cm. long and 3mm. in diameter, the edges being brought as close together as possible without contact. The glass rod was removed, and the cylinder placed vertically before and below the slit of the spectroscope and maintained at any desired temperature below the melting point of platinum by means of a constant electric current. The gas was conveyed by a glass tube into the lower end of the cylinder, and after becoming heated rose in a stream before the slit. Radiation from the platinum cylinder was carefully kept out by a metallic screen. The dispersing piece was a 60° prism of fluor spar. This was mounted upon the table of a spectrometer reading to 5″ and was kept in the position of minimum deviation by an automatic device. The customary lenses were replaced by concave silver-on-glass mirrors. The bolometer employed to investigate the spectrum was of extremely delicate construction. The sensitive resistance required less than 2 seconds to attain aconstant temperature, since within this length of time after the beginning of a throw, the galvanometer needle was again completely at rest. The galvanometer itself was probably the most sensitive ever constructed. It is described by the writer in a previous article. (See *Wied. Ann.*, Bd. 48, p. 272).

The high sensibility was attained principally by the use of a number of extremely short magnets, the magnetic moment of the system being thereby reduced in a much smaller ratio than the moment of inertia. The needle gave a throw of 1 mm. for 3.3×10^{-11} Amp., the extreme theoretical sensibility of the bolometer being 5 millionths of a degree centigrade.

The temperature of the current of gas was measured by means of a Platinum, Platinum-Rhodium couple. The superior limit to the temperature employed was about 1250° C.

The gases examined were Air, Oxygen, Carbon dioxide and Water-vapor. With air and oxygen, no radiation was detectedexcept that due to impurities consisting of water or carbon dioxide. The spectrum of carbon dioxide showed a

sharp maximum of radiation at about wave-length 4.75 μ, corresponding to a deviation (minimum) of 29° 21′. Near this point the galvanometer deflection rose rapidly from near 0 to 330 mm.

In the case of water vapor, a maximum of considerable intensity (180 mm.) appeared at wave-length 2.75μ, and several smaller maxima also occur in such a way as to suggest a band-spectrum. The spectrum of the gases from a Bunsen-burner, taken at a point 4 cm. above the last discernible trace of flame, exhibited all the principal maxima of CO_2 and water vapor as did also, with increased intensity, the spectrum of the flame itself. The intensity of these maxima was found to decrease rapidly with the temperature of the gas. Nevertheless, at 284° C. the principal maximum in the water-spectrum was still present, and at 73° C. the CO_2 maximum was clearly distinguishable. In the latter case, the highest temperature of the platinum cylinder was 120°. At this temperature Paschen considers it out of the question that any trace of dissociation can have taken place in CO_2. The same continuous spectrum is, however, present as in the Bunsen flame at a temperature of 1460° C. This fact indicates that a chemical process is not a necessary condition for the radiation from the Bunsen burner flame, or from the gases which arise from it. Moreover, at the highest possible temperature of the platinum cylinder, the CO_2 maximum was about ¾ as intense as in the Bunsen flame, and the simplest conclusion is that they are due to the same cause, namely, a simple heating.

From these observations, Paschen concludes that gases can emit discontinuous spectra in virtue of their temperature alone, thus contradicting the deductions of Pringsheim. He does not hesitate to admit, however, that chemical action may be the means of intensifying the emitted radiation.

Upon reading Paschen's paper, one cannot fail to be impressed with the great delicacy of his apparatus, and the accuracy of his results. The conclusions which he draws from them, will be interesting in the light of future work which is bound to be done on this subject. They appear, to be fair interpretations of the observed facts, and to be free from objectionable hypotheses. As to whether Water vapor and Carbon dioxide satisfy Kirchhoff's Law, no conclusion can be drawn, as their absorption spectra have not been studied. It is to be hoped that Mr. Paschen will push his investigations in this direction, so as to throw, if possible, more light upon this interesting question.

ROBERT R. TATNALL.

The Object-Glass Grating.—We have received the following letter from Mr. L. E. Jewell:

Since writing the paper on the "Object Glass Grating" in ASTRONOMY AND ASTRO-PHYSICS for January, a further study of the subject has shown the desirability of some changes in the methods proposed for constructing such a grating.

When a grating for use has been constructed upon a plate of plane glass, with parallel surfaces, then instead of the method suggested it would be much better to take another plate of plane parallel glass, and cement it to the film side of the grating. This process offers several advantages, the chief being that the photographic film is protected from injury in use, and if there is any irregularity in either film or plates it can be corrected by an optician after the grating has been made.

Another change thought desirable, would do away with some of the greatest photographic difficulties, *i. e.*, the construction of a photographic slit perfectly straight and symmetrical. The difficulty may be avoided by using as a slit the space between two wires stretched vertically by weights and rendered parallel by

fitting into adjacent grooves of similar screws which are pressed against the wires at the top and bottom. Screens with some soft, sticky, opaque material at their edges can be pressed against the outer edges of the wires, without affecting their parallelism or distance apart, so as to exclude all light except that which comes through the aperture between the wires. Furthermore the wires may be smoked, thus coating them with a thin black deposit of carbon which will not affect the slit unfavorably, but instead will prevent the reflection of light at the edges of the wires.

Greater usefulness may be given to an object-glass grating constructed in the manner suggested, if at the time it is copied from the original a circular disc be interposed so as to leave vacant a circular space (of a size to be determined by the diameter of the object-glass and other considerations) in the centre of the grating. In this way a fairly good image of the star may be obtained, so that in photographing stellar spectra requiring a long exposure, we may scratch a straight line across the middle of the photographic plate deep enough to cut through the film, adjust the plate so that the scratch will be parallel to the equator, and then having an aperture in the back of the camera in which an eye-piece may be mounted, we can so guide the telescope with the eye as to keep the star's image on the scratch in the film of the plate, while by slightly altering the clock rate we can lengthen the image in right ascension so as to produce a spectrum of the desired width. Likewise the scratch may be adjusted parallel to a right ascension circle and the star moved along the scratch by gradually altering the declination of the telescope.

The many photographs of the heavens that have been made have shown the necessity of controlling the motion of the telescope by hand so as to keep it constantly pointed in the direction desired, imperfections of clock-work and the adjustments of an equatorial necessitating some such control. This would seem to be nearly as desirable in photographing stellar spectra as in photographing the stars themselves.

Where the chief use of a telescope is the photographing of stellar spectra, the telescope tube should not be of circular cross-section, but of such shape as to permit the use of as long plates as the spectra on each side of the normal will cover with the dispersion it is desired to use. LEWIS E. JEWELL.

The Great Cluster in Hercules.—Professor Scheiner contributes an interesting popular article on this cluster in the December number of *Himmel und Erde*, in which the drawings of Herschel, Secchi, Lord Rosse, Trouvelot, and Harrington are compared with the results of photographic researches at Potsdam. The three radiating "canals" of some of these observers are shown to have no real existence. Narrower canals and outlying rows of stars, or "arms" can be traced to some extent on the photographs, but they are not more striking than similar fortuitous arrangements obtained by spatter-work. Counts of stars in circular areas of suitable radius justify the supposition that the general form of the cluster is globular. Professor Scheiner deprecates the writing of popular articles containing speculations which run ahead of scientific demonstration.

Change in the Brightness of Nova Aurigæ.—According to M. Bigourdan, of the Paris Observatory, a change of about one magnitude in the brightness of this unique variable took place in October and November of the past year. The light fell off notably between the middle of October and the 8th of November, then increased for a few days, but the brightness of Oct. 10 was not regained.

Chromatic Aberration of Telescopes.—An interesting discussion of various matters pertaining to telescopes, at the November meeting of the Royal Astronomical Society, is reported in the December number of the *Observatory*. Mr. H. Dennis Taylor read a paper on the amount of light lost for purposes of definition by the effect of chromatic aberration, and arrived at the conclusion that the loss is very considerable, varying from 9 per cent. in the case of a 6-inch refractor to 50 per cent. in that of the new 28-inch Greenwich equatorial. Owing to the great focal length of the Lick telescope as compared with its aperture, the loss in that case is small,—only 27 per cent.

As the total amount of light in the image is independent of the aberration, it is evident that light is not lost in the sense that it would be if absorbed by the object-glass. The effect of the loss of light referred to in a technical way by Mr. Taylor is equivalent to bad definition in the ordinary way of regarding the subject. Mr. Taylor's method of procedure seems to have been the following: starting with the color curve of an objective, obtained either by observation, or by computation from the known constants of the glass, the diameters of the circles of chromatic aberration at any given plane (assumed to pass through the focus) can be found, and hence what rays fall within and what without the limits of the theoretical star-disc. Evaluating the physiological effect of the rays so divided, by means of Abney's curve of luminous intensity in the normal solar spectrum, the percentage of light thrown outside the star-disc is obtained, and this light is regarded as lost.

While it is hardly fair to judge Mr. Taylor's paper by the brief abstract given in the report, the results already mentioned, taken in connection with ordinary experience, lead to the belief that he must have set too high a value on the scattered rays. Dawes' rule gave for the distance between double stars just admitting resolution by a telescope

$$\frac{4''.56}{a},$$

(a being the aperture), while theory gives

$$\frac{5''.52}{a},$$

the actual telescope thus exceeding its theoretical requirements. The difference is no doubt due to the fact that the illumination at the edges of the star-disc is very feeble, so that its full size is not seen except in the case of very bright stars. Dawes' formula was deduced from observations made with small telescopes, but the great telescopes of modern times have shown themselves capable of working up to nearly the theoretical limit. Hence it would seem that practically chromatic aberration is less injurious to definition than Mr. Taylor finds it to be.

Assuming that Mr. Taylor's theory is correctly outlined above, the explanation of its apparent variance with the results of experience may be as follows: Consideration of the color curve of a well-corrected objective shows that the greater part of the "lost" light falls very close to the circumference of the theoretical star-disc, which is after all a somewhat arbitrary limit; hence the effect of this light, which numerically would rank with widely scattered light of the same intensity, is merely to slightly enlarge the image. It is further to be observed that the distribution of light in the superposed circles of chromatic aberration is not uniform.

The Astronomer Royal stated that the color curve of the new 28-inch objective, obtained by observations with a McClean star spectroscope, seemed to differ from the color curves of other large instruments. The minimum focus (at a point

halfway between D and E) seemed to be higher than usual, although the outstanding color in ordinary observation was entirely satisfactory to the eye.

Attention may here be called to the fact that the McClean spectroscope is not suitable for such determinations, as the considerable chromatic errors of the apparatus and of the eye vitiate the results. A slit spectroscope should be used, and the adjustments of the instrument for each part of the spectrum determined beforehand by means of solar lines. In general the minimum focus of a large telescope lies somewhat higher than the place mentioned by the Astronomer Royal (D). In the Lick telescope it is at about λ 5650. J. E. K.

Notes on Stellar Spectra.—In *Astronomische Nachrichten*, No. 2963, Espin called attention to the fact that the $H\beta$ hydrogen line is very bright in φ Persei. Shortly afterward I examined the spectrum of that star and found $H\alpha$ to be very much brighter than $H\beta$, as it is at present. I supposed the bright $H\alpha$ line was well known, but am unable to find any statement that it has been observed by others.

Professor Pickering and Mrs. M. Fleming have announced in various places that the Draper Catalogue photographs show the $H\beta$ line to be bright in the stars ψ Persei, π Aquarii, $\varkappa$ Draconis, v Cygni, v Sagittarii, χ Ophiuchi and Cord. Gen. Catal., 7191. I have examined the spectra of all these stars, and in every case the $H\alpha$ hydrogen line is bright and very much easier to observe than the $H\beta$ line. The hydrogen lines in the violet of these stars are probably dark, for the most part; but photographs of the regions $H\beta$ to $H\delta$ of some of these stars show peculiarities of considerable interest, and make a detailed investigation desirable. Thus, in φ Persei the $H\gamma$ and $H\delta$ lines consist each of two narrow bright lines about four tenth-metres apart upon a background but slightly darker than the ordinary continuous spectrum. In v Cygni $H\beta$, $H\gamma$ and $H\delta$ consist of narrow bright lines upon broad and nearly dark backgrounds.

Miss A. C. Maury found from the Draper Catalogue plates that $H\beta$ and possibly $H\gamma$ in the spectrum of Pleione consist each of a narrow bright line on a dark background. The $H\alpha$ line is bright in this star. Other observers have possibly called attention to that fact, but I am unable to find any such reference. Qualitative investigations of these spectra cannot be made advantageously by photography with the 36-inch telescope, owing to the large chromatic aberration of the great lenses in the blue and violet.

The spectrum of *Alcyone* is always classed as Secchi's type I, or A, as in the Draper Catalogue; that is, a spectrum containing dark (and usually rather broad) hydrogen lines. I have observed this star visually and found the $H\alpha$ hydrogen line to be bright. It is not very intense, but in good seeing is easily visible with the 36-inch telescope. There is a narrow dark line in contact with it on the side of smaller wave-length, and possibly a still finer one on the side of greater wave-length. I do not think this line can be seen with a 12 inch telescope.

A photograph of the portion of the spectrum between $H\beta$ and K shows $H\beta$, $H\gamma$, $H\delta$ and H to be dark, as was expected, and a few additional fine dark lines. —W. W. C. in *Publications of the Astronomical Society of the Pacific*, No. 32.

The existence of both bright and dark hydrogen lines in the same spectrum seems to be the most remarkable and mysterious spectroscopic anomaly yet discovered. So far it has been observed by Professor Campbell only, but few other observers have the facilities which are probably required for the purpose. An explanation of the phenomenon in question was suggested by Professor Frost at the Astro-Physical Congress in Chicago, but it seemed inadequate to account for more than a very slight contrast in the intensity of the lines.

The bright $H\alpha$ line in the spectrum of *Pleione* was noted by Keeler (ASTRONOMY AND ASTRO-PHYSICS, No 114, p. 352).

The Planetary Nebula SD. — 12°, 1172.—In No. 32 of the *Publications of the Astronomical Society of the Pacific*, Professor Campbell gives some details regarding the spectrum of this nebula, which was discovered by Mrs. Fleming on the Harvard plates. The nebula is about 15″ in diameter and it has a nucleus or central star of the 9th magnitude. The three monochromatic images corresponding to the three strongest nebular lines at λ 5007, λ 4959 and λ 4862, were measured with a micrometer, the slit being open, and their diameters were found to be 11″, 9″ and 14″ respectively. This would show that the hydrogen envelope is more extensive than the others, but it is not stated whether allowance was made for the effect of chromatic aberration on the diameter of the image at the slit-plate. The differences are perhaps too great to be accounted for in this way.

The nebula resembles the great nebula of Orion in the relative strength of the hydrogen lines, and as it occurs in the same nebulous region, a possible common origin of the two objects is suggested.

New Variable Star.—Wolsingham Observatory Circular No. 38 announces that photographs taken with the Compton telescope show that the star Es.-Birm. 57, R. A., $11^h\ 39^m\ 58^s$, Dec. $+56°\ 23'$ (1855), Mag. 9.5, is variable. The star is now of the 8.5 magnitude, type III.

Stars with Remarkable Spectra.—Mr. Espin's latest list (in *A. N. 3200*) contains over one hundred stars, most of which are of type III. The spectrum of R Coronæ contains bright hydrogen lines, a yellow line (D_3?) and numerous other bright lines and bands. Some changes were observed, indicating perhaps that the spectrum is a double one. The spectrum of T Coronæ is now certainly not nebular, as Lockyer stated it to be in 1889, and probably it is of type III. The $H\gamma$ line may be bright.

Observations of a Solar Prominence, and Reversal of the D_3 Line.—Mr. W. E. Woods, of Washington, D. C., sends us sketches of a prominence observed by him on the north-east limb of the Sun, on Dec. 18, 1893. The prominence was not specially active, as indicated by the appearance of the hydrogen lines. Slight differences were suspected in the form of the prominence as observed in different lines of the spectrum. These differences are indicated in the sketches. The latter part of the letter, relating to a supposed double reversal of the D_3 line, is given below.

"A feature of the observation was the apparent reversal of the D_3 line. This is imperfectly indicated in the sketch. I had grave doubts as to the fact, at first, and returned to the D_3 line again and again, after observations of the other lines, and finally decided from four separate observations that the reversal occurred only at or very near the base of the prominence; of this fact, I am positive. I am not aware, either by experience of the past two years with the spectroscope, or the work of others, of the reversal of D_3, but it was so decided in this morning's observation that I thought it well to note it. A feature of this reversal was the unsymmetrical situation of the dark line with respect to the edges of the bright line. Its situation was toward the more refrangible side of the line; dividing the width (not height) of the bright line into four parts, it would occupy

the third fourth. This dark line was unusually sharp, black and fine. The day was remarkable for the purity and quiet of the atmosphere and the definition the best in two years' work. The grating has 24,000 lines to the inch."

Architect's Office, U. S. Capitol, Washington, D. C. W. E. WOODS.

Unit of Velocity in the Line of Sight.—In seems very unfortunate that there should be such a lack of uniformity among spectroscopists as to the unit employed in expressing velocities in the line of sight. Within the past year results expressed in four different units have been published in scientific journals. These units are the German geographical mile, the *lieue geographique*, the English mile and the kilometer. The last named is the only one which can lay claim to international acceptance, and the object of this note is to urge upon spectroscopists the uniform adoption of the kilometer in expressing velocities in the line of sight.

It appears almost superfluous to add other reasons for this simple extension of the use of the metric system in the interest of uniformity. We mention but one or two of the other points in favor of the kilometer.

The displacements of the spectral lines, from which the velocities in the sight line are deduced, are now universally reckoned in the metric system, either the ten-millionth or the millionth of a millimeter being employed. Symmetry demands the use of the same system in the other member of the proportion, and it may be recalled incidentally that Newcomb's official statement of the velocity of light is in kilometers per second.

The magnitude of the kilometer is also very convenient with the present accuracy of the determinations of stellar velocities in the sight line, and this convenience will continue as our accuracy increases. Decimals of a kilometer do not ordinarily need to be used in a single determination and the tenths of a kilometer are at present all that is required for the mean value of a series of determinations. It is difficult to see what actual advantage is gained by the employment of a larger unit even in such cases as that of Nova Aurigæ, where the uncertainty of a determination must for ordinary instruments amount to several kilometers, inasmuch as an idea of the measure of accuracy may be gained from the probable error or from the deviations of the separate determinations from the mean.

It is certain that neither the German geographical mile nor the *lieue geographique* will be adopted in English speaking countries, and equally certain that the English mile will not find acceptance on the Continent. The kilometer, however, ought to be alike acceptable to all parties.

In this connection may we allude to the fact that the metric units are real English words, and that the leading dictionaries prefer the spelling *meter* to *metre* and similarly for the other units with like endings.

While mentioning this the need of a proper term and abbreviation for the ten-millionth of the millimeter recurs to mind. The word "tenth-meter" is objectionable because it does not adapt itself to other languages. "X-meter" is very clumsy and "Ångstrmöm unit" no less so. Suggestions for a suitable international name and abbreviation or symbol for this unit have been in order for a long time. EDWIN B. FROST.

[It is to be hoped that Professor Frost's suggestion will meet with general approval. In this connection it seems proper to add that in the translation of Scheiner's *Spectralanalyse der Gestirne* on which he is engaged, Professor Frost has adopted the kilometer as the unit of velocity in the line of sight.]

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR MARCH.

H. C. WILSON.

Mercury during March will be passing between the earth and the Sun, as may be seen from the diagram in our last number, page 71. For the first two or three days the planet will be visible in the evening just after sunset. In order to see it one must look toward the west, just a little above the horizon. On March 14, 2^h 18^m A. M., Mercury will be in conjunction with the Sun, and after that time it will be morning planet.

Venus will be morning star and rapidly come out from the rays of the Sun. She will increase rapidly in brilliancy so that none can mistake her, greatest brilliancy being attained on the 22nd of March. Venus will be in conjunction with the waning moon, 12° 28′ north, March 4 at 9^h 39^m P. M. Central time.

Mars rises about 4 o'clock in the morning and is at such a southern declination that there will be little opportunity for observation of this planet in northern latitudes during March. It is in the constellation Sagittarius and moving eastward. Mars will be in conjunction with the Moon, 4° 44′ north, March 1 at 11^h 29^m P. M. and again March 30 at 11^h 38^m P. M.

Jupiter will be in good position for observation in the early evening. His position southwest of the Pleiades is so well known by this time that in needs no mention. His motions during March will be eastward. Jupiter will be in conjunction with the Moon, 4° 40′ south, March 11 at 2^h 40^m P. M.

Saturn rises in the evening and will be in good position for observation after midnight. For the position of this planet in the constellation Virgo see the chart in our last number. Saturn will be in conjunction with the Moon, 4° 24′ north, March 23 at 3^h 01^m A. M.

Uranus is in the constellation Libra, southeast from Saturn (see chart page 73), and may be observed after midnight. Uranus will be in conjunction with the Moon, 3° 39′ north, at 6^h 12^m P. M., March 24.

Neptune will be in good position for observation during the early evening in March. The position of this planet in Taurus is unchanged from last month.

The asteroid ***Juno*** is in the constellation Libra about 5° northeast of the star β. It is making the turn of the loop in its apparent path and after the middle of the month will move westward.

PLANET TABLES FOR MARCH.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Mar. 5	23 51.8	+ 2 33	6 44 A. M.	12 57.6 P. M.	7 11 P. M.
15	23 27.0	+ 0 02	5 50 "	11 53.7 A. M.	5 57 "
25	23 06.7	− 4 33	5 09 "	10 54.1 "	4 40 "

VENUS.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Mar. 5	21 20.5	− 7 36	4 54 A. M.	10 27.0 A. M.	4 00 P. M.
15	21 25.0	− 8 59	4 24 "	9 52.0 "	3 20 "
25	21 42.5	− 9 23	4 04 "	9 30.3 "	2 56 "
MARS.					
Mar. 5	19 00.6	− 23 16	3 42 A. M.	8 07.5 A. M.	12 33 P. M.
15	19 31.0	− 22 32	5 31 "	7 58.5 "	12 26 "
25	20 01.0	− 21 27	3 16 "	7 49.1 "	12 22 "
JUPITER.					
Mar. 5	3 32.4	+ 18 25	9 18 A. M.	4 37.8 P. M.	11 58 P. M.
15	3 38.7	+ 18 49	8 43 "	4 04.8 "	11 27 "
25	3 45.8	+ 19 15	8 09 "	3 32.6 "	10 57 "
SATURN.					
Mar. 5	13 34.1	− 6 54	9 02 P. M.	2 37.8 A. M.	8 14 A. M.
15	13 32.0	− 6 40	8 19 "	1 56.3 "	7 33 "
25	13 29.5	− 6 24	7 36 "	1 14.6 "	6 53 "
URANUS.					
Mar. 5	14 51.5	− 16 01	10 57 P. M.	3 55.0 A. M.	8 53 A. M.
15	14 50.8	− 15 58	10 16 "	3 14.9 "	8 13 "
25	14 49.8	− 15 53	9 36 "	2 34.6 "	7 33 "
NEPTUNE.					
Mar. 5	4 37.8	+ 20 35	10 13 A. M.	5 43.0 P. M.	1 13 A. M.
15	4 38.2	+ 20 37	9 34 "	5 04.1 "	12 35 "
25	4 38.9	+ 20 39	8 55 "	4 25.5 "	11 56 P. M.
THE SUN.					
Mar. 5	23 05.0	− 5 51	6 31 A. M.	12 11.5 P. M.	5 52 P. M.
15	23 42.2	− 1 55	6 13 "	12 08.9 "	6 05 "
25	0 18.7	+ 2 01	5 55 "	12 05.9 "	6 17 "

Phases and Aspects of the Moon.

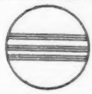
	Central Time. d	h m
Apogee	Mar. 1	10 06 A. M.
New Moon	" 7	8 18 A. M.
First Quarter	" 14	12 28 P. M.
Perigee	" 16	11 18 P. M.
Full Moon	" 21	8 11 A. M.
Last Quarter	" 29	2 28 A. M.
Apogee	" 29	6 42 A. M.

Approximate Central Standard Times when the Great Red Spot will cross the Central Meridian of Jupiter.

		h m			h m			h m
Mar.	1	6 39 P. M.	Mar.	12	10 46 P. M.	Mar.	22	9 06 P. M.
	3	12 26 A. M.		13	6 38 "		23	4 58 "
	3	8 18 P. M.		15	12 25 A. M.		24	10 46 "
	4	4 10 "		15	8 17 P. M.		25	6 37 "
	5	9 57 "		16	4 09 "		26	12 25 A. M.
	6	7 49 "		17	9 56 "		27	8 17 P. M.
	7	11 36 "		18	5 48 "		28	4 08 "
	8	7 28 "		19	11 35 "		29	9 56 "
	9	3 19 "		20	7 27 "		30	5 47 "
	10	9 07 "		21	3 19 "		31	11 35 "
	11	4 59 "						

Jupiter's Satellites for March.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I.  r *

III.  d * r *

II.  d r *

IV.  No Eclipse.

Configuration at 7ʰ for an Inverting Telescope.

Day.	West		East.
1	·4 3·	○	·2 ·1 ●
2	·43· 1·	○	2·
3	·3 ·2 ·4	○	·1
4	1· ·3	○	·4 ·2 ●
5		○	1· 2· ·3 ·4
6	2· ·1	○	·3 ·4
7	·2	○	1· 3· ·4
8	3· ·1	○	·2 4·
9	○ 1· 3·	○	2· 4·
10	·3 2·	○	·1 4·
11	·31·	○	4· ·2 ●
12	4	○	·1 ·3 2·
13	4· ·12·	○	·3
14	4· ·2	○	1· 3·
15	4· ·1	○	3· ·2
16	4· 3·	○	1· 2·
17	·4 ·3 2·	○	·1 ●
18	.4 ·3 1· ·2	○	
19	·4	○	·3·1 2·
20	1· ·4 2·	○	·3
21	·2	○	1· ·4 3·
22	·1	○	3· ·2 4·
23	3·	○	1· 2· ·4
24	3· 2·	○	·4 ·1 ●
25	·3 ·21·	○	4·
26		○	·3 ·1 ·2 4·
27	○ 2· .1	○	·3 4·
28	·2	○	·1 4· 3·
29	·1 4·	○	·2 3·
30	4· 3·	○	1· 2·
31	4· 3· 2· ·1	○	

Phenomena of Jupiter's Satellites.

Central Time.

	h	m			
Mar. 1	12	13	P. M.	III	Tr. In.
	2	21	"	III	Tr. Eg.
	5	28	"	III	*Sh. In.
	6	18	"	I	*Oc. Dis.
	7	28	"	III	*Sh. Eg.
	9	45	"	I	*Ec. Re.
2	3	27	"	I	Tr. In.
	4	44	"	I	Sh. In.
	5	41	"	I	*Tr. Eg.
	6	57	"	I	*Sh. Eg.
	8	48	"	II	*Tr. In.
	11	13	"	II	Tr. Eg.
	11	21	"	II	Sh. In.
3	1	44	A. M.	II	Sh. Eg.
	12	47	"	I	Oc. Dis.
	4	14	P. M.	I	Ec. Re.
4	9	57	A. M.	I	Tr. In.
	11	12	"	I	Sh. In.
	12	10	P. M.	I	Tr. Eg.
	1	26	"	I	Sh. Eg.
	3	03	"	II	Oc. Dis.
	5	28	"	II	Oc. Re.
	5	37	"	II	*Ec. Dis.
	7	54	"	II	*Ec. Re.
5	2	12	A. M.	III	Oc. Dis.
	4	20	"	III	Oc. Re.
	7	17	"	I	Oc. Dis.
	7	29	"	III	Ec. Dis.
	9	17	"	III	Ec. Re.
	10	43	"	I	Ec. Re.
6	4	26	"	I	Tr. In.
	5	41	"	I	Sh. In.
	6	40	"	I	Tr. Eg.
	7	55	"	I	Sh. Eg.
	10	10	"	II	Tr. In.
	12	35	P. M.	II	Tr. Eg.
	12	40	"	II	Sh. In.
	3	03	"	II	Sh. Eg.
7	1	46	A. M.	I	Oc. Dis.
	5	12	"	I	Ec. Re.
	10	56	P. M.	I	Tr. In.
8	12	10	A. M.	I	Sh. In.
	1	09	"	I	Tr. Eg.
	2	23	"	I	Sh. Eg.
	4	24	"	II	Oc. Dis.
	6	49	"	II	Oc. Re.
	6	55	"	II	Ec. Dis.
	9	12	"	II	Ec. Re.
	4	25	P. M.	III	Tr. In.
	6	34	"	III	*Tr. Eg.
	8	16	"	I	*Oc. Dis.
	9	29	"	III	*Sh. In.
	11	30	"	III	Sh. Eg.
	11	41	"	I	Ec. Re.
9	5	25	"	I	Tr. In.
	6	39	"	I	*Sh. In.
	7	39	"	I	*Tr. Eg.
	8	52	"	I	*Sh. Eg.
	11	32	"	II	Tr. In.
10	1	57	A. M.	II	Tr. Eg.
	1	59	"	II	Sh. In.
Mar.10	4	22	A. M.	II	Sh. Eg.
	2	46	"	I	Oc. Dis.
	6	10	"	I	*Ec. Re.
11	11	55	"	I	Tr. In.
	1	08	P. M.	I	Sh. In.
	2	08	"	I	Tr. Eg.
	3	21	"	I	Sh. Eg.
	5	45	"	II	*Oc. Dis.
	8	10	"	II	*Oc. Re.
	8	14	"	II	*Ec. Dis.
	10	31	"	II	Ec. Re.
12	6	25	A. M.	III	Oc. Dis.
	8	35	"	III	Oc. Re.
	9	15	"	I	Oc. Dis.
	11	29	"	III	Ec. Dis.
	12	39	P. M.	I	Ec. Re.
	1	18	"	III	Ec. Re.
13	6	25	A. M.	I	Tr. In.
	7	37	"	I	Sh. In.
	8	38	"	I	Tr. Eg
	9	50	"	I	Sh. Eg
	12	54	P. M.	II	Tr. In.
	3	18	"	II	Sh. In.
	3	19	"	II	Tr. Eg.
	5	41	"	II	*Sh. Eg.
14	3	45	A. M.	I	Oc. Dis.
	7	08	"	I	Ec. Re.
15	12	54	"	I	Tr. In.
	2	05	"	I	Sh. In.
	3	08	"	I	Tr. Eg.
	4	19	"	I	Sh. Eg.
	7	07	"	II	Oc. Dis.
	6	32	"	II	Oc. Re.
	9	32	"	II	Ec. Dis.
	11	49	"	II	Ec. Re.
	8	41	P. M.	III	*Tr. In.
	10	15	"	I	Oc. Dis.
	10	51	"	III	Tr. Eg.
	1	30	A. M.	III	Sh. In.
16	1	37	"	I	Ec. Re.
	3	32	"	III	Sh. Eg.
	7	24	P. M.	I	*Tr. In.
	8	34	"	I	*Sh. In.
	9	38	"	I	*Tr. Eg.
	10	48	"	I	Sh. Eg.
17	2	16	A. M.	II	Tr. In.
	4	36	"	II	Sh. In.
	4	42	"	II	Tr. Eg.
	7	00	"	II	Sh. Eg.
	4	45	P. M.	I	Oc. Dis.
	8	06	"	I	*Ec. Re.
18	1	54	"	I	Tr. In.
	3	03	"	I	Sh. In.
	4	08	"	I	Tr. Eg.
	5	16	"	I	Sh. Eg.
	8	29	"	II	*Oc. Dis.
19	1	07	A. M.	II	Ec. Re.
	10	42	"	III	Oc. Dis.
	11	15	"	I	Oc. Dis.
	12	53	P. M.	III	Oc. Re.
	2	35	"	I	Ec. Re.

Mar. 19	3 30 "	III	Ec. Dis.
	5 20 "	III	Ec. Re.
20	8 24 A. M.	I	Tr. In.
	9 32 "	I	Sh. In.
	10 38 "	I	Tr. Eg.
	11 45 "	I	Sh. Eg.
	3 39 P. M.	II	Tr. In.
	5 55 "	II	*Sh. In.
	6 05 "	II	*Tr. Eg.
	8 19 "	II	*Sh.Eg.
21	5 45 A. M.	I	Oc. Dis.
	9 03 "	I	Ec. Re.
22	2 54 "	I	Tr. In.
	4 01 "	I	Sh. In.
	5 08 "	I	Tr. Eg.
	6 14 "	I	Sh. Eg.
	9 51 "	II	Oc. Dis.
	2 26 P. M.	II	Ec. Re.
23	12 15 A. M.	I	Oc. Dis.
	12 58 "	III	Tr. In.
	3 09 "	III	Tr. Eg.
	3 32 "	I	Ec. Re.
	5 30 "	III	Sh. In.
	7 33 "	III	Sh. Eg.
	9 24 P. M.	I	Tr. In.
	10 30 "	I	Sh. In.
	11 38 "	I	Tr. Eg.
24	12 43 A. M.	I	Sh. Eg.
	5 03 "	II	Tr. In.
	7 14 "	II	Sh. In.
	7 29 "	II	Tr. Eg.
	9 38 "	II	Sh. Eg.
	6 45 P. M.	I	*Oc. Dis.
	10 01 "	I	Ec. Re.
25	3 54 "	I	Tr. In.
	4 58 "	I	Sh. In.
	6 08 "	I	*Tr. Eg.
	7 12 "	I	*Sh. Eg.
	11 14 "	II	Oc. Dis.
Mar. 26	3 44 A. M.	II	Ec. Re.
	1 15 P. M.	I	Oc. Dis.
	3 02 "	III	Oc. Dis.
	4 30 "	I	Ec. Re.
	5 14 "	III	Oc. Re.
	7 31 "	III	*Ec. Dis.
	9 22 "	III	Ec. Re.
27	10 24 A. M.	I	Tr. In.
	11 27 "	I	Sh. In.
	12 38 P. M.	I	Tr. Eg.
	1 40 "	I	Sh. Eg.
	6 26 "	II	*Tr. In.
	8 33 "	II	*Sh. In.
	8 52 "	II	*Tr. Eg,
	10 57 "	II	Sh. Eg.
28	7 45 A. M.	I	Oc. Dis.
	10 59 "	I	Ec. Re.
29	4 54 "	I	Tr. In.
	5 56 "	I	Sh. In.
	7 08 "	I	Tr. Eg.
	8 09 "	I	Sh. Eg.
	12 37 P. M.	II	Oc. Dis.
	5 03 "	II	Ec. Re.
30	2 15 A. M.	I	Oc. Dis.
	5 18 "	III	Tr. In.
	5 28 "	I	Ec. Re.
	7 30 "	III	Tr. Eg.
	9 31 "	III	Sh. In.
	11 35 "	III	Sh. Eg.
	11 24 P. M.	I	Tr. In.
31	12 25 A. M.	I	Sh. In.
	1 38 "	I	Tr. Eg.
	2 38 "	I	Sh. Eg.
	7 50 "	II	Tr. In.
	9 52 "	II	Sh. In.
	10 16 "	II	Tr. Eg.
	12 16 P. M.	II	Sh. Eg.
	8 45 "	I	*Oc. Dis.
	11 57 "	I	Ec. Re.

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc. denotes occultation; Tr., transit of the satellite; Sh., transit of the shadow; * Visible at Washington.

A Partial Eclipse of the Moon will occur on March 21. It will not, however, be visible in the United States except in the extreme western part just as the Moon is setting. It will be visible in Alaska, the Pacific Ocean and Asia. At the middle of the eclipse 0.248 of the Moon's diameter will be obscured. The following are the elements of the eclipse as given by the *American Ephemeris*:

Greenwich mean time of opposition in right ascension March 21, $1^h\ 27^m\ 17^s.1$.

Sun's right ascension	$0^h\ 03^m\ 24^s.38$	Hourly motion	$9^s.10$
Moon's right ascension	12 03 24 .38	Hourly motion	120 .73
Sun's declination	$0°\ 22'\ 10''.1$N	Hourly motion	$0'\ 59''.2$N
Moon's declination	0 36 09 .5N	Hourly motion	16 29 .3S
Sun's equa. hor. parallax	8 .6	Sun's semi-diameter	16 02 .9
Moon's equa. hor. parallax	58 10 .5	Moon's " "	15 50 .4

TIMES OF THE PHASES:

	Gr. Mean Time. h m	Central Time. h m	Pacific Time. h m
Moon enters penumbra	March 21, 11 57.4 A. M.	5 57.4 A. M.	3 57.4 A. M.
Moon enters shadow	1 25.3 P. M.	7 25.3 "	5 25.3 "
Middle of the eclipse	2 20.6 "	8 20.6 "	6 20.6 "
Moon leaves shadow	3 15.7 "	9 15.7 "	7 15.7 "
Moon leaves penumbra	4 43.7 "	10 43.7 "	8 43.7 "

Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc.]

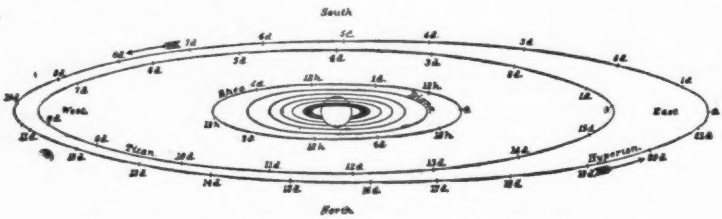


DIAGRAM OF THE APPARENT ORBITS OF SATURN'S SATELLITES.

MIMAS.

		h		
Mar.	2	3.2	A. M.	E
	3	1.8	"	E
	4	12.4	"	E
	4	11.0	P. M.	E
	9	4.8	A. M.	W
	10	3.4	"	W
	11	2.0	"	W
	12	12.6	"	W
	12	11.3	P. M.	W
	18	3.6	A. M.	E
	19	2.3	"	E
	20	12.9	"	E
	20	11.5	P. M.	E
	21	10.1	"	E
	26	3.8	A. M.	W
	27	2.5	"	W
	28	1.1	"	W
	28	11.7	P. M.	W
	29	16.3	"	W

ENCELADUS.

		h		
Mar.	1	12.0	midn.	E
	3	8.9	A. M.	E
	4	5.8	P. M.	E
	6	2.7	A. M.	E
	7	11.5	"	E
	8	8.4	P. M.	E
	10	5.3	A. M.	E
	11	2.2	P. M.	E
	12	11.1	"	E
	14	7.9	A. M.	E
	15	4.8	P. M.	E
	17	1.7	A. M.	E
	18	10.6	"	E
	19	7.5	P. M.	E

ENCELADUS CONT.

		h		
Mar.	21	4.3	A. M.	E
	22	1.2	P. M.	E
	23	10.1	"	E
	25	7.0	A. M.	E
	26	3.9	P. M.	E
	28	12.7	A. M.	E
	29	9.6	"	E
	30	6.5	P. M.	E
Apr.	1	3.4	A. M.	E

TETHYS.

		h		
Mar.	2	11.9	A. M.	E
	4	9.2	"	E
	6	6.5	"	E
	8	3.8	"	E
	10	1.1	"	E
	11	10.4	P. M.	E
	13	7.7	"	E
	15	5.0	"	E
	17	2.3	"	E
	19	11.6	A. M.	E
	21	8.9	"	E
	23	6.2	"	E
	25	3.5	"	E
	27	12.8	"	E
	28	10.1	P. M.	E
	30	7.4	"	E

DIONE.

		h		
Mar.	3	5.5	P. M.	E
	6	11.2	A. M.	E
	9	4.8	"	E
	11	10.5	P. M.	E
	14	4.2	"	E
	17	9.9	A. M.	E
	20	3.5	A. M.	E

DIONE CONT.

		h		
Mar.	22	9.2	P. M.	E
	25	2.9	"	E
	28	8.6	A. M.	E
	31	2.2	"	E

RHEA.

		h		
Mar.	2	9 9	A. M.	E
	6	10.3	P. M.	E
	11	10.7	A. M.	E
	15	11.1	P. M.	E
	20	11.5	A. M.	E
	24	11.8	P. M.	E
	29	12.2	"	E

TITAN.

		h		
Mar.	4	12.9	P. M.	W
	8	2.8	"	S
	12	9.9	A. M.	E
	16	7 4	"	I
	20	10.9	"	W
	24	12.9	P. M.	S
	28	7.9	A. M.	E
Apr.	1	5.4	"	I

HYPERION.

		h		
Mar.	5	6.7	P. M.	S
	10	10.7	"	E
	17	7.3	A. M.	I
	22	2.5	P. M.	W
	26	11 8	"	S
Apr.	1	4 0	A. M.	E

IAPETUS.

		h		
Feb.	28	1.0	A. M.	W
Mar.	19	9.9	P. M.	S
Apr.	9	9.0	A. M.	E

Occultations Visible at Washington.

Date 1894.		Star's Name.	Magnitude.	IMMERSION Washington M. T.		IMMERSION Angle f'm N pt.	EMERSION Washington M. T.		EMERSION Angle f'm N pt.	Duration.	
				h	m	°	h	m	°	h	m
Mar.	16	λ Cancri	6	14	47	62	15	20	340	0	33
	18	37 Leonis	6	15	49	57	16	14	1	0	25
	20	β Virginis	4	10	52	173	11	49	269	0	57

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A. .... $0^h\ 52^m\ 32^s$
Decl. .... $+81°\ 17'$
Period .... $2d\ 11^h 50^m$

Mar.	3	7 A. M.
	5	7 P. M.
	8	6 A. M.
	10	6 P. M.
	13	6 A. M.
	15	6 P. M.
	18	6 A. M.
	20	5 P. M.
	23	5 A. M.
	25	5 P. M.
	28	5 A. M.

ALGOL.

R. A. .... $3^h\ 1^m\ 1^s$
Decl. .... $+40°\ 32'$
Period .... $2d\ 20^h 49^m$

Mar.	6	4 A. M.
	9	1 "
	11	10 P. M.
	14	7 "
	17	3 "
	20	12 noon
	23	9 A. M.
	26	6 "
	29	3 "

λ TAURI.

R. A. .... $3^h\ 54^m\ 35^s$
Decl. .... $+12°\ 11'$
Period .... $3d\ 22^h\ 52^m$

Mar.	1	3 P. M.
	5	2 "
	9	1 "
	13	noon
	17	11 A. M.
	21	9 "
	25	8 "
	29	7 "

R CANIS MAJORIS.

R. A. .... $7^h\ 14^m\ 30^s$
Decl. .... $-16°\ 11'$
Period .... $1d\ 3^h\ 16^m$

Mar.	1	10 P. M.
	3	1 A. M.
	4	5 "
	5	8 "
	6	11 "
	7	2 P. M.
	8	6 "
	9	9 "
	10	12 midn.
	12	3 A. M.
	13	7 "
	14	10 "
	15	1 P. M.
	16	4 "
	17	8 "
	18	11 "

R CANIS MAJ., CONT.

20	2 A. M.
21	5 "
22	9 "
23	12 noon.
24	3 P. M.
25	7 "
26	10 "
28	1 A. M.
29	4 "
30	8 "
31	11 "

S CANCRI.

R. A. .... $8^h\ 37^m\ 39^s$
Decl. .... $+19°\ 26'$
Period .... $9d\ 11^h\ 38^m$

Mar.	6	1 P. M.
	16	1 A. M.
	25	1 P. M.

S ANTLIÆ.

R. A. .... $9^h\ 27^m\ 30^s$
Decl. .... $-28°\ 09'$
Period .... $0d\ 7^h\ 47^m$

Mar.	1	5 P. M.
	2	12 midn.
	3	11 P. M.
	4	11 "
	5	10 "
	6	9 "
	7	9 "
	8	8 "
	9	7 "
	10	7 "
	11	6 "
	12	5 "
	13	12 midn.
	14	12 "
	15	11 P. M.
	16	11 "
	17	10 "
	18	9 "
	19	9 "
	20	8 "
	21	7 "
	22	7 "
	23	6 "
	24	5 "
	25	5 "
	26	12 midn.
	27	11 P. M.
	28	11 "
	29	10 "
	30	9 "
	31	9 "

δ LIBRÆ.

R. A. .... $14^h\ 55^m\ 06^s$
Decl. .... $-\ 8°\ 05'$
Period .... $2d 07^h 51^m$

Mar.	1	5 A. M.
	3	1 P. M.
	5	9 "
	8	5 A. M.
	10	1 P. M.
	12	9 "
	15	5 A. M.
	17	12 noon
	19	8 P. M.
	22	4 A. M.
	24	12 noon
	26	8 P. M.
	29	4 A. M.
	31	11 "

U CORONÆ.

R. A. .... $15^h\ 13^m\ 43^s$
Decl. .... $+32°\ 03'$
Period .... $3d\ 10^h\ 51^m$

Mar.	4	1 A. M.
	7	12 noon
	10	11 P. M.
	14	10 A. M.
	17	9 P. M.
	21	7 A. M.
	24	6 P. M.
	28	5 A. M.

U OPHIUCHI.

R. A. .... $17^h\ 10^m\ 56^s$
Decl. .... $+\ 1°\ 20'$
Period .... $0d\ 20^h\ 08^m$

Mar.	1	2 A. M.
	2	10 P. M.
	3	6 "
	4	2 "
	5	10 A. M.
	6	6 "
	7	2 "
	7	11 P. M.
	8	7 "
	9	3 "
	10	11 A. M.
	11	7 "
	12	3 "
	12	11 P. M.
	13	8 "
	14	4 "
	15	12 noon
	16	8 A. M.
	17	4 "
	17	12 midn.
	18	8 P. M.
	19	4 "
	20	1 "
	21	9 A. M.
	22	5 "
	23	1 "

U OPHIUCHI CONT.

Mar.	23	9 P. M.
	24	5 "
	25	1 "
	26	9 A. M.
	27	6 "
	28	2 "
	28	10 P. M.
	29	6 "
	30	2 "
	31	10 A. M.

Y CYGNI.

R. A.............. $20^h\ 47^m\ 40^s$
Decl................ $+ 34° 15'$
Period........... $1d\ 11^h\ 57^m$

Mar.	2	2 P. M.
	4	2 A. M.
	5	2 P. M.
	7	2 A. M.
	8	2 P. M.
	10	2 A. M.
	11	2 P. M.
	13	2 A. M.
	14	2 P. M.

Y CYGNI CONT.

Mar.	16	2 A. M.
	17	2 P. M.
	19	2 A. M.
	20	2 P. M.
	22	2 A. M.
	23	2 P. M.
	25	2 A. M.
	26	2 P. M.
	28	1 A. M.
	29	1 P. M.
	31	1 A. M.

Test Objects for Small Telescopes.

[From Clark and Sadler's "Star Guide."]

Name of Objects.	R. A.		Decl.		Distance.	Position Angle.	Magnitudes.		Aperture of telescope
	h	m	°	'	"	°			
					DIVIDING TESTS.				
14 Orionis...........	5	02	+ 8	20	1.15	203	5.8	6.0	4
4 Lyncis..............	6	12	+ 59	25	0.95	101	6.2	7.5	5
Σ 1333.................	9	11	+ 35	56	1.69	42	6.7	7.0	3
OΣ 229................	10	42	+ 41	42	0.84	330	6.5	7.5	6
Σ 1606.................	12	05	+ 40	32	1.21	338	6.2	7.0	4
Cor. Bor. 1.........	15	14	+ 27	15	1.22	308	5.6	6.1	4
μ^2 Boötis............	15	20	+ 37	47	0.78	104	6.5	7.8	6
					108.4	172	4.5		
λ Ophiuchi..........	16	25	+ 2	14	1.65	44	4.4	5.4	3
OΣ 313................	16	29	+ 40	21	0.99	152	7.0	7.6	5
Cephei 83............	20	59	+ 56	13	1.62	349	6.4	6.8	3
					DEFINING TESTS.				
ψ^2 Orionis...........	5	21	+ 2	59	2.66	324	5.4	9.0	4
42 Orionis...........	5	30	− 4	55	1.73	218	5.5	9.2	6
κ Leonis.............	9	18	+ 26	41	3.36	205	5.1	10.1	6
					10.	65		11.5	
49 Leonis............	10	29	+ 9	14	2.39	158	6.2	8.4	3
84 Virginis..........	13	37	+ 4	07	3.56	234	5.7	8.0	3
6 Serpentis..........	15	15	+ 1	08	2.28	13	4.7	9.4	6
η Draconis..........	16	22	+ 61	46	5.26	142	2.8	9.0	4
68 Herculis.........	17	13	+ 33	14	4.41	62	5.1	10.1	5
70 Ophiuchi........	18	00	+ 2	32	2.05	20	4.3	6.2	3
60 Cygni.............	20	57	+ 45	42	2.71	165	5.5	9.5	5
					SPACE-PENETRATING TESTS.				
40 Cassiopeiæ.....	1	29	+ 72	27	53.3	237	6.0	10.9	5
λ Geminorum.....	7	12	+ 16	45	9.5	33	3.5	9.8	3
θ Cancri.............	8	25	+ 18	29	60.8	60	5.5	10.4	4
δ Cancri..............	8	38	+ 18	37	42	113	5.0	11.8	6
υ Ursæ Maj........	11	12	+ 33	43	7.0	147	3.5	9.6	3
ρ Boötis.............	14	27	+ 30	52	53	334	3.6	11.7	6
5 Ursæ Min.........	14	28	+ 76	12	56.4	129	4.8	10.5	4
5 Serpentis..........	15	13	+ 2	13	10.7	38	4.8	10.0	3
54 Ophiuchi........	17	29	+ 13	15	21.6	75	6.0	11.0	5
110 Herculis........	18	41	+ 20	26	61.2	96	5.0	12.0	6
					44.7	92		11.0	

New Asteroid 1893 AP.—A new asteroid of the 12th magnitnde was discovered by Charlois at Nice, Dec. 6, 1893. It was in R. A., $4^h\ 21^m.8$, Decl. $+ 18° 05'$. Asteroid No. 334 was named Chicago by Professor M. Wolf while he was in attendance at the astronomical congress at the Columbian Exposition.

NEWS AND NOTES.

It is possible that some subscribers may not receive the February number of this publication, either because they have not renewed their subscription, or because we have failed to notify them of the expiration of the same. We will be greatly aided in mailing if every subscriber, who has not already done so, will at once advise us whether or not he desires the publication continued.

Professor Schaeberle's Photographs of the Corona of the Total Solar Eclipse, April 16, 1893.—In the October number of this magazine we published a large plate showing the interior corona as obtained by Professor J. M. Schaeberle while in South America. In connection with that plate it was stated that owing to the delay in the reproduction of the plates, it was necessary to hold the long exposure photograph for publication in the next number. The photogravure company who had the work in charge after a further delay of a month, finally said it was impossible to bring out the details expected in the long exposure photograph of the outer corona and so gave up the task wholly. Another photogravure company in the city of Milwaukee tried to produce the second plate and likewise failed. Neither of those in charge of this particular work was satisfied with the plate secured, and after two further trials, at considerable expense, followed by complete failures, it was decided to give up further attempts.

Gold Medal for Professor Burnham.—Cablegrams from London announce the award of the Gold Medal of the Royal Astronomical Society to Professor S. W. Burnham of the University of Chicago for his discoveries and micrometrical measures of double stars and for his researches on the orbital motions of Binary Systems. This news is especially welcome to American astronomers, and will be favorably received throughout the scientific world, for no observer either living or dead has contributed more to this important branch of modern astronomy than has Professor Burnham, whose discoveries of new and very close pairs have created an epoch in the history of Double Star Astronomy. The discovery of double stars, begun by Sir William Herschel more than a century ago, and since continued by William and Otto Struve, Herschel and Mædler, Dawes and Dembowski, was regarded twenty-five years ago as practically exhausted. But the genius of Burnham working with only a six-inch telescope soon brought to light hundreds of close pairs never before detected, and opened the way to later discoveries of priceless value. Professor Burnham afterwards secured for a time the use of the Dearborn 18-inch refractor, and the Madison 15-inch, and thus extended the list of measures and new discoveries. His work at the Lick Observatory is too recent to need recalling to the readers of this Journal, but it may not be inappropriate to remark that his own stars now number nearly 1300, and include the most rapid and interesting pairs in the heavens.

It is understood that these stars will be made the object of special attention at the Yerkes Observatory, and that they will be carefully followed until their orbits are accurately known. Professor Burnham's catalogue of his new stars and his general catalogue of all the important stars in the northern hemisphere are to be printed among the first volumes issued by the Yerkes Observatory, and will constitute works on Double Star Astronomy which are destined to be "aere perennius."

The high honor conferred upon Professor Burnham is a tribute to pure science which will be fully appreciated by all American astronomers, but it is especially gratifying to the intimate friends of this modest, unselfish and renowned observer.

Other American astronomers who have received this Medal of recent years are:—Professor Simon Newcomb, Professor Asaph Hall, Dr. B. A. Gould, Professor E. C. Pickering, and Dr. G. W. Hill. Last year the Gold Medal was awarded to Dr. H. C. Vogel, and the preceding year to Professor G. H. Darwin.

No new comets have been discovered since October of last year. Brooks' comet *c* 1893 was barely visible in our 16-inch telescope on the night of Jan. 26. It was too faint to admit of measurement.

We looked very carefully for Holmes' comet on the same night but could find no trace of it. On the night of Jan. 12 we took a photograph of the region in which this comet should be, giving an exposure of an hour with the new 6-inch camera. There is a slightly oval stain, 20′ in diameter, on the negative just where the comet ought to be. It has no central condensation and is so suspiciously like a dirty water stain that we hesitate to say anything about it without verification, which we have as yet been unable to get.

Discovery of Comet *b*, 1893.—Notes on the independent discovery of this comet by Messrs. Rordame, Quenisset, Miller, Johnson, Roso de Luna, Sperra, have been printed in these *Publications*, 1893, pages 154-5. A full account of Mr. Sperra's observations is given in ASTRONOMY AND ASTRO-PHYSICS, 1893, page 757. The Committee on the Comet-Medal, having carefully considered the case, and having asked the advice of the editors of the leading astronomical journals, has adopted the following resolutions:

I. That a copy of the Comet-Medal shall be struck, having the *obverse* as usual and the *reverse* blank, and that on the reverse of this copy shall be engraved the words:

To Commemorate the Discovery of Comet b, 1893.

II. That this Medal shall be preserved in the cabinet of the Astronomical Society of the Pacific, and no award made for the discovery of this comet.

III. That a copy of No. 32 of the Society's *Publications* shall be sent to each of the gentlemen named above.

Committee on the Comet-Medal.

EDWARD S. HOLDEN,
J. M. SCHAEBERLE,
CHAS. BURCKHALTER.

[Dated]

December 1, 1893.—*Publications of the Astronomical Society of the Pacific, No. 32.*

An International Cipher Code for Aurora Observations.—Dr. M. A. Veeder of Lyons, New York, read a paper at the Chicago Meteorological Congress, August, 1893, bearing title: "An International Cipher Code for Correspondence Respecting Aurora and Related Conditions." The points made in that paper are:

1. Selection of such points as are important for observation and code-correspondence.

2. The place should involve more than the data for preservation that are incidentally secured.

3. There should be a system of intercommunication that would promptly

furnish the views of sun-spots, magnetic storms and auroras continuously if possible.

4. There ought to be a daily synoptic chart to go with the ordinary daily meteorological data.

Any persons interested in taking meteorological observations in connection with the system now recommended by him should correspond with him for instructions. His excellent work in this direction is gaining world-wide attention.

Quick Adjustment of the Equatorial.—On the hypothesis that the maker has placed the axes at right angles with each other, and the tube at right angles with the declination axis, the following adjustments remain to be made by the astronomer, viz.:

The adjustment of the finder.

The bringing of the polar axis into the merdian.

The setting of the circles.

The elevating of polar axis to the latitude of the place.

In the first place when the telescope *head* is placed upon the pier make the polar axis cut the meridian *west* of *north* and *east* of *south.*

To adjust the finder bring a star into the center of large glass, then by means of the adjusting screws attached to finder bring the star to center of field of latter. The finder is then adjusted.

To bring the polar axis into the meridian, turn the instrument to zenith (approximately), place a lever of precision north and south across the lower (eye) end of tube (which is at right angles to line of collimation), turn instrument on decl. axis until the bubble comes to center, (clamp in decl. axis), turn the level east and west across end of tube. Turn instrument on polar axis until bubble comes to center. The instrument now points to the zenith. Set the hour circle vernier to 0 hours and 12 hours respectively. Set the decl. circle to the latitude of the place (the latter is only approximately correct). Loose in decl. axis and turn the objective down to take in a star (Sirius) about to cross the meridian. Bring the star to center of the field, hold it there by shifting north end of instrument to the *east* in the pier until the sidereal time indicates star's R. A. The polar axis is now in the meridian.

To elevate the polar axis to the latitude of the place, turn the instrument to a star of known decl. (Sirius). If the axis is at the proper elevation the decl. circle will read the star's declination. If the circle does not thus read (and this is usually the case) note the differences between the circle reading and the star's decl. as given in the catalogue. Shift the decl. circle *one-half* of this difference, bring the tube again into the meridian by means of the hour circle, and set the decl. circle to read the latitude of the place. (Do this by turning instrument on decl. axis). Place the level north and south across the end of tube as before; bring the bubble to center by raising or turning (as the case may be) the north end of instrument, by means of the adjusting screws.

Test your adjustment by setting instrument for some star and see if it appears in field on correct sidereal times.

By means of the preceding method the writer has put an equatorial into good adjustment in a single evening. L. W. U.

Volume XXV of the Astronomical Observatory of Harvard College has been received. It contains the comparison of positions of stars between 49° 50′ and 55° 10′ of north declination in 1855.0, and observed with the meridian circle

during the years 1870 to 1884, by William A. Rogers under the direction of Joseph Winlock and Edward C. Pickering.

Table I gives the values of the systematic corrections required to reduce the co-ordinates given by different catalogues to the system of the Astronomische Gesellschaft. Table II is a comparison of the computed with the observed positions. Then follows the separate results of 8612 zone stars in right ascension and declination for the Epoch of 1875.0 and reduced to the system of the Gesellschaft, separate results of zone stars for the proper motions μ and μ' and other minor features.

The values of the proper motions given in this volume are to be considered as provisional, and the values of the reductions to the system of this zone catalogue are given in table I. These numbers are the results of three approximations. In the first it was assumed that all residuals exceeding 0.5^s in right ascension and $5''$ in declination were due to proper motion and were omitted in the first discussion. In the second approximation these values of the proper motions were introduced in a second discussion of systematic deviations from the system of the zone catalogue and new values of the proper motions were then obtained. At this point in the discussion, it became possible to distinguish with considerable certainty between residuals which were due to a real progressive movement in the stellar position and those which were due to the accidental error of observation. The list of selected stars was revised at this point, and residuals exceeding the values in right ascension and declination above given which appeared to belong to the class of accidental errors of observation were still excluded from the list. In the third approximation the same order of procedure was followed, and values given are the sum of the three sets of corrections obtained in this manner.

This volume is a work of years, by Professor Rogers, a part of which only is represented in its printed pages. The skill and care represented in its results are best known to those astronomers acquainted with the details of his elaborate work.

Volume XXIX of the Harvard College Observatory Publications contains six papers as follows:

1. Meteorological and other observations made at Willows, California, in connection with the total solar eclipse of Jan. 1, 1889, by W. Upton and A. L. Rotch.
2. Longitude of Smith College Observatory by Mary E. Byrd and Mary W. Whitney, from which is derived the following result:

   The longitude of Smith College Observatory west of Greenwich is

   $$4^h\ 50^m\ 33^s.096 \pm 0^s.044$$

3. Photometric observations of Asteroids by Henry M. Parkhurst.
4. Observations of Variable Stars by Henry M. Parkhurst.
5. Magnitude of bright stars north of $+70°$.
6. Relative places of β Persei and companion stars, from observations with the Meridian Circle by Arthur Searle.

An Extraordinary Phenomena.—On the morning of the 20th of December, a celestial body of extraordinary appearance was observed by the citizens of North and South Carolina and Virginia. We quote a description received from Wilmington, N. C., through the kindness of Mr. E. S. Martin.

The weight of the evidence goes to show this remarkable fact that the lumin-

ous body passed from about West, S. of E., until it reached a point some 15° about the eastern horizon when it seemed to pause and remain stationary apparently for the space of 15^m or 20^m and then disappeared. This can be accounted for (if a meteor) by the path being along the line of sight from the observer, and if a spectroscope had been used this could have been determined and its rate of motion.

That it was a tremendous body is evident from all the reports; those here and elsewhere reporting its appearance as about the size of a large table 6′ to 8′ in diameter, others as large as a hogshead, and others a wheel of 6′ to 8′ diameter; that it was brilliantly white throwing off sparks—moved apparently over this city towards the eastern horizon a little north of where the Sun rose; that the sound of its motion through the air could be heard; that it left an intensely thick stream of vapor nearly its entire way from west to east which was visible for 30^m and even after sunrise for some time, and which slowly drifted, and towards the east assumed a zigzag shape. According to some very intelligent witnesses, after remaining apparently stationary, as heretofore mentioned, it seemed to explode though no sound was heard and a rain of stars (as they expressed it) fell from it towards the horizon and near the point where the Sun had just risen.

The time of passing this point was about $6^h 30^m$ A. M., E. S. T., Dec. 20, 1893 and the body remained visible until $6^h 45^m$ when it exploded as aforesaid.

The Chicago Academy of Sciences—Section of Mathematics and Astronomy, Jan. 3rd, 1894.—The annual meeting was held at the Dearborn Observatory, Evanston, Ill., Professor Hough, President, in the chair. After the transaction of routine business, the President introduced Professor Malcolm McNeill, of Lake Forest University, who read a paper on "*The Life and Times of Tycho Brahe.*" The speaker began by recalling the conditions which had surrounded Tycho in his youth, and gave an account of the circumstances which led him to become an astronomer, of which one of the most important was the celebrated new star that blazed forth in Cassiopeia. He then discussed with care the instruments employed by Tycho, and the degree of precision of which they were capable. It was shown that some of them had the form of circles mounted on polar axes, which enabled the observer to find the declination of the body and the arc by which it was out of the meridian, the latter being measured on a circle at right angles to the axis of the movable circle. Professor McNeill also pointed out the rough similarity of Tycho's sights to those employed by modern astronomers, and observed that in dealing with his arcs Tycho had corrected for the co-called "eccentricity of the sextant." The speaker gave an interesting account of the influence of Astrology on Tycho, and of his official relations to the King of Denmark, and in turn sketched the mystic influence of Tycho upon Kepler and his successors. At this time Astrology was hardly distinguished from Astronomy. Professor McNeill did not think Tycho's rejection of the Copernican system was very remarkable, considering the roughness and inaccuracy of the data then available; but he said Tycho's real greatness rests upon his fine observations which proved so valuable to Kepler, and upon the energetic effort made to eliminate all possible sources of error. He called attention to the great importance of Tycho's work in forming a new catalogue of stars independent of that of Ptolemy, which had been generally employed by preceding astronomers. In conclusion the speaker gave the important sources to which astronomers are indebted for their knowledge of Tycho, and commended the work of Dr. Dreyer on the "*Life of Tycho*

Brahe" as a reliable and interesting contribution to the literature of the subject.

Dr. Kurt Laves of the University of Chicago read the second paper of the evening on the "*Application of the so-called Lunar Equation in the Motion of the Earth for the Determination of Certain Important Constants in Astronomy.*" After having derived the equation and discussed the constants entering into it, he called attention to the fact that the method of using the Lunar Equation for a determination of the solar parallax is unavailable because the constant of Nutation which is introduced into the equation by the mass of the Moon, produces a very large probable error in the determination of Solar Parallax. By a total differentiation of the Lunar Equation it follows that the equation is to be applied only for a determination of the mass of the Moon, and by introducing the resulting mass of the Moon into the equations which follow for the Precession and Nutation, we get a very reliable method for finding by a theoretical process the Constant of Nutation, the probable error of which is $\pm\ 0''.012$. This probable error is about the same as that in the determination of the Nutation from the right ascensions of stars near the pole. The weak point which has hitherto rendered the results inexact lies in the large probable error of $P = 6''.52 \pm 0''.02$ But the author stated that a more accurate value of this constant of the Lunar Equation could be deduced from careful micrometrical measures of small planets which come near to the Earth at the time of opposition than could ever be found from observations of the Sun. He stated that Dr. Gill had recently obtained a very good value of P by means of heliometer observations of small planets, and said that it would be of high importance if similar measures could be undertaken in America. Dr. Laves gave the value of the Constant of Nutation resulting from the above method as

$$N = 9''.255 \pm 0''.012.$$

In the discussion which followed, Professor Hough, Professor Burnham and several others took part.

The present officers were re-elected for the ensuing year, after which the meeting adjourned. T. J. J. SEE, Recorder.

Astronomical and Physical Society of Toronto, Canada.—Dec. 26th meeting; Chairman, Mr. Robert B. Ellis.

Letters read from Dr. Joseph Morrison, F. R. A. S., of Washington, promising papers; from Professor E. C. Pickering, LL. D., Director of Harvard College Observatory, accepting honorary membership.

Mr. Mungo Turnbull of Toronto was elected an active member.

Librarian G. G. Pursey reported reception of a copy of Professor J. E. Keeler's finely illustrated paper on "Physical Observations of Mars during 1892;" sets of *The Sidereal Messenger* and of ASTRONOMY AND ASTRO-PHYSICS for several years; also several volumes on various subjects, gifts of members.

Mr. George E. Lumsden presented a photograph of some beautiful frost-tracery on a window-pane; also drawings of Jupiter and Venus delineated at the telescope during bright sunshine.

Mr. A. F. Miller reported seven noteworthy groups of sunspots, three in the northern hemisphere and four in the southern. He said these spots could be shown well by projection on a sheet of white paper, simply using a telescope made with ordinary spectacle lenses. Messrs. Miller, A. Elvin, R. Dewar and W. Collins said the spectacle-lenses could be made round by paring with strong scissors, danger of breakage being avoided by holding them under water.

Mr. Lindsay called attention to the valuable scientific papers made available by the recent binding of the reports received from the Royal and other societies.

Miss A. A. Gray read from ***Popular Astronomy*** an interesting article by Professor E. E. Barnard of Lick Observatory, describing the peculiar appearances presented on the mornings of Oct. 21, 22 and 23, 1893, by the Brooks' comet (*d* 1893). John A. Copland contributed a paper on the same subject by Mr. E. W. Maunder, F. R. A. S., of Greenwich Observatory, published, with copies of Mr. Barnard's negatives, in ***The London Daily Graphic.***

Meeting of Jan. 9, 1894.—Fourth annual metting. Chairman, Vice-President John A. Paterson, M. A., delivered the annual address, eloquently reviewing the past year's work.

Mr. F. H. Young, M. A., of Belleville, was elected an associate member.

On motion of Mr. Thomas Lindsay, seconded by Mr. D. Geo. Ross, the following officers and members of council were unanimously re-elected for the year 1894: Honorary President, Hon. G. W. Ross, LL. D., Minister of Education; President, Mr. Charles Carpmael, F. R. A. S., F. R. S. C., Director of the Toronto Observatory; Vice-Presidents, Mr. Larratt W. Smith, D. C. L., Q. C., and Mr. John A. Paterson, M. A.; Treasurer, Mr. James Todhunter; Assistant Treasurer, Miss S. L. Taylor; Corresponding Secretary, Mr. G. E. Lumsden; Recording Secretary, Mr. Charles P. Sparling; Assistant Recording Secretary, Miss A. A. Gray; Librarian, Mr. G. G. Pursey: Assistant Librarian, Miss Jeane Pursey. The council is composed of the executive officers and Messrs. E. A. Meredith, LL. D., A. Elvins, A. F. Miller, A. Harvey and D. J. Howell.

Mr. John A. Copland was re-elected British and foreign correspondent.

Having signified their assent, which was done in terms flattering to the society, Professor E. C. Pickering, LL. D., Director of the Harvard College Observatory, and Professor S. P. Langley, LL. D., Secretary of the Smithsonian Institution, were elected honorary members, and Professor James Keeler, LL. D., Director of the Allegheny Observatory was elected a corresponding member.

Mr. Pursey laid on the table various publications including the latest reports of the British Astronomical Association and The Astronomical Society of the Pacific, as well as a presentation advance copy of the first volume of "Webb's Celestial Objects for Common Telescopes," just published by, and received direct from Messrs. Longmans, Green & Co., of London. In addition to the compliment thus paid by the publishers, Rev. T. E. Espin, F. R. A. S., editor of the new edition, in his preface, credits the Society with having suggested valuable features to be introduced into the book. The first volume deals entirely with the solar system; the second with the stars.

Mr. Todhunter, the treasurer, presented an encouraging annual report, showing that the assets of the Society, including telescopes, globes, books, etc., had been valued at nearly $1,100; that there are 103 active members, 22 life, honorary and corresponding members, and 6 associate members, and that, after meeting all engagements, there was a small balance in bank.

Mr. Sparling reported that during 1893, twenty-five regular meetings had been held, with an average attendance that indicated a well-sustained interest, summer and winter, in the work of the Society.

On motion of the treasurer, the annual fee for active lady members was placed at $1, and the life membership fee for a lady member at $10.

Miss A. A. Gray read a list of phenomena.

A striking display of aurora in the northeastern sky at 6:10 P. M. on Wednesday, 3rd January, was described by Messrs. John A. Copland, G. G. Pursey, Andrew Elvins, J. Hollingworth, of Beatrice, and a lady member. It was also observed by Dr. Larratt W. Smith, Mr. W. T. Moore and many other citizens. Mr. Copland said when he first noticed the display, all the streamers were deep crimson, seeming to roll into each other, and then shoot toward the zenith. At 6:20, the deep red color was rent in two places by white bars, a brightly white streamer being toward the east. White and red kept interchanging, with crimson predominating, until 6:30, after which the display gradually faded.

JOHN A. COPLAND.

PUBLISHER'S NOTICES.

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James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill.; Jos. S. Ames, Johns Hopkins University, Baltimore, Md.

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